TRANSACTIONS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XXI. PART III., FOR THE SESSION 1855–56.

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CONDITIONS OF AWARD

OF THE

KEITH PRIZE.

This Prize, the interest of a sum which now amounts to about L.630 left by the late Alexander Keith, Esq., of Dunnottar, will be awarded by the President and Council of the Royal Society of Edinburgh, on the following conditions:—

I. The author of the best paper on a scientific subject (preference being in all cases given to a paper containing an important discovery in science made in any part of the world), communicated, in the first instance, to the Royal Society during the sessions 1857-8, 1858-9, or any two succeeding sessions, shall be entitled to the biennial interest of the Keith Fund, accruing in the respective periods.

II. The form of the Prize shall be a Gold Medal, of not more than Fifteen Guineas value. The remainder of the sum shall be given in money, to be spent in Plate or otherwise, at the discretion of the receiver.

III. The award being duly intimated to the receiver of the Prize, he is to apply forthwith to the Treasurer of the Society for payment of it; and, failing to do so within six months of the date of the intimation, he shall forfeit the money, but shall be entitled to receive the Medal.

KEITH PRIZE.

The Keith Medal and Prize for the Biennial period terminating in November 1855, has been awarded by the Council to Dr Thomas Anderson, Professor of Chemistry in the University of Glasgow, for his Papers "On the Products of the Destructive Distillation of Animal Substances," and "On the Crystalline Bodies obtained from Opium," both printed in the Transactions of the Society.

BRITISH MUSEUM

CONDITIONS

OF THE

AWARD OF THE FIRST MAKDOUGALL-BRISBANE PRIZE.

1. The First Brisbane Prize will be adjudicated by the Council of the Royal Society of Edinburgh as early as practicable during the Session 1856–57. The competing Essays are to be given in to the Secretary of the Society on or before the 1st December 1856.

2. The competition is open to all men of science.

3. The Essays may be either anonymous or otherwise. In the former case they must be distinguished by mottoes, with corresponding sealed billets superscribed with the same motto, and containing the name of the author.

4. The first award will be in the form of a Gold Medal of Fifteen Guineas value. In future Biennial Adjudications the value of the prize will be greater.

5. The subject proposed by the Council for the Prize of 1856–57 is the following:—

A Biographical Notice of a Scotchman eminent in Science; including an estimate of the influence and importance of his writings and discoveries.

As instances of such Biographies which still remain to be supplied, the Council would specify the following names:—Maclaurin, Black, Monro *Primus* and *Secundus*, several of the family of Gregory, Sir James Hall, Jameson. The earlier volumes of the Transactions of the Royal Society contain several specimens of able Biographies of the kind referred to. The Council are anxious to see a continuation of the series.

6. The Council impose no restriction as to the length of the Essays, which may be, at the discretion of the Council, read at the Ordinary Meetings of the Society. They wish also to leave the free disposal of the manuscripts to the Authors; a copy, however, being deposited in the archives of the Society, unless the paper shall be published in the Transactions.

The Keith Prize (value about £60), for the best Paper communicated to the Society, is awarded in alternate years with the Brisbane Prize.

XXIII.—*On a Problem in Combinations.* By The Rev. Philip Kelland, M.A., Professor of Mathematics in the University of Edinburgh.

(Read 3d December 1855.)

Several years ago, when discussing the question of the distribution of the stars, a problem occurred to Professor Forbes, which, simple as it is, appears to have escaped notice prior to that time. Having been consulted as to its solution, I communicated my results to Professor Forbes, who has inserted one of them in his paper printed in the Philosophical Magazine for 1850, vol. xxxvii., p. 425. But for the very ingenious application which Professor Forbes has there made of it, the problem might probably not be worth recurring to. As it is, I have thought it would not be altogether uninteresting to give the complete solution.

The Problem is as follows:—There are n dice, each of which has p faces, p being not less than n; it is required to find the number of arrangements which can be formed with them, 1°, So that no two show the same face; 2°, That no three show the same face; 3°, That no four do so, and so on.

1. The number of arrangements in which no two show the same face is easily seen to be the same as the number of permutations of the p faces, taken n together; and is therefore $p\,(p-1)\,(p-2)\,\ldots\,(p-n+1)$.

2. Remove the dice A and B, and cover the face 1 of the remainder. The number of arrangements which these can now form, omitting the covered face, and no two showing the same face, will be—

$$\begin{aligned}&(p-1)\,(p-2)\,\ldots\,(p-1-\overline{n-2}+1)\\ =&(p-1)\,(p-2)\,\ldots\,(p-n+2).\end{aligned}$$

Place with each of these the dice A and B, showing face 1, and you have the arrangements in which the dice A and B, and these alone, show face 1. The same applies to each of the other faces. Consequently there are $p\,(p-1)\,\ldots\,(p-n+2)$ arrangements in which the dice A and B, and these alone, show the same face. The same is true of every other pair of dice. Hence the number of arrangements in which two, and two only, show the same face, is—

$$n\,\frac{(n-1)}{1\,.\,2}\,p\,(p-1)\,\ldots\,(p-n+2).$$

3. Remove the dice A, B, C, D, and cover the faces 1 and 2 of the others. The number of arrangements which can now be formed in which no two show the same face is—

$$\begin{aligned}&(p-2)\,(p-3)\,\ldots\,(p-2-\overline{n-4}+1)\\ =&(p-2)\,(p-3)\,\ldots\,(p-n+3).\end{aligned}$$

By placing A and B showing face 1, and C and D showing face 2, with each of these, you have the arrangements in which two particular duplications of the dice A, B, C, D, and those only, occur. Now the number of arrangements of faces 1 and 2, on dice A, B, C, D, is the number of permutations, all together, of four things, two of one kind and two of another, or, $\frac{4.3.2.1}{1^2.2^2}$.

Hence $$\frac{4.3.2.1}{2^2}(p-2)(p-3)\ldots.(p-n+3)$$

is the number of arrangements in which two duplications occur of faces 1 and 2, and on dice A, B, C, D. The same is true of any other pair of faces; consequently the number of arrangements in which two duplications are found on the dice A, B, C, D, but on no others, nor any repetition of the faces shown on these four dice, is—

$$\frac{p(p-1)}{1.2}\cdot\frac{4.3.2.1}{2^2}(p-2)(p-3)\ldots.(p-n+3).$$

In like manner, any other four dice form the same number of arrangements; and hence the total number of arrangements in which two duplications and no more occur, is—

$$\frac{4.3.2.1}{2^2}\cdot\frac{n(n-1)(n-2)(n-3)}{1.2.3.4}\cdot\frac{1}{1.2}p(p-1)\ldots.(p-n+3).$$

4. Similarly, the number of arrangements in which three duplications, and three only, occur without any other repetition, is,—

$$\frac{6.5.4.3.2.1}{2^3}\cdot\frac{n(n-1)\ldots.(n-5)}{1.2\ldots.6}\cdot\frac{1}{1.2.3}p(p-1)\ldots.(p-n+4),$$

and the law of formation is evident.

5. We may now write the number of arrangements in which no triplication occurs, in the following form:—

$$\begin{aligned}&p(p-1)\ldots.(p-n+1)+\frac{n(n-1)}{1.2}p(p-1)\ldots.(p-n+2)\\&+\frac{4.3.2.1}{2^2}\cdot\frac{n(n-1)\ldots.(n-3)}{1.2.3.4}\cdot\frac{1}{1.2}p(p-1)\ldots.(p-n+3)\\&+\frac{6.5\ldots.1}{2^3}\cdot\frac{n(n-1)\ldots.(n-5)}{1.2\ldots..6}\cdot\frac{1}{1.2.3}p(p-1)\ldots.(p-n+4)+\&c.\\&=p(p-1)\ldots.(p-n+1)\left\{1+\frac{n(n-1)}{p-n+1}\cdot\frac{1}{2}+\right.\\&\left.\frac{n(n-1)\ldots.(n-3)}{(p-n+1)(p-n+2)}\cdot\frac{1}{1.2}\cdot\frac{1}{2^2}+\frac{n(n-1)\ldots.(n-5)}{(p-n+1)(p-n+2)(p-n+3)}\cdot\frac{1}{1.2.3}\cdot\frac{1}{2^3}+\&c.\right\}\end{aligned}$$

6. This series may be exhibited as the solution of a differential equation, but it is doubtful whether, with our present knowledge, we can simplify its form. We obtain the differential equation thus:—

Let $$u=1+\frac{n(n-1)}{p-n+1}x+\frac{n(n-1)\ldots(n-3)}{(p-n+1)(p-n+2)}\cdot\frac{x^2}{1.2}+\&c.$$

Then
$$\frac{du}{dx}=\frac{n(n-1)}{p-n+1}+\frac{n(n-1)\ldots\ldots(n-3)}{(p-n+1)(p-n+2)}x$$
$$+\frac{n(n-1)\ldots.(n-5)}{(p-n+1)\ldots.(p-n+3)}\frac{1}{1.2}x^2+\&c.$$

$\therefore$
$$\frac{d}{dx}\left(x^{p-n+1}\frac{du}{dx}\right)=n(n-1)x^{p-n}+\frac{n\ldots.(n-3)}{(p-n+1)}x^{p-n+1}$$
$$+\frac{n(n-1)\ldots.(n-5)}{(p-n+1)(p-n+2)}\frac{x^{p-n+2}}{1.2}+\&c.$$
$$=n(n-1)z^{2n-2p}+\frac{n\ldots.(n-3)}{p-n+1}z^{2n-2p-2}+\&c.$$

when
$$x=\frac{1}{z^2}.$$

Hence
$$z^{2p-n-2}\frac{d}{dx}\left(x^{p-n+1}\frac{du}{dx}\right)=\frac{d^2}{dz^2}\left(z^n u\right);$$

or
$$z^{2p-n-2}\left\{z^{-2p+2n-2}\frac{d^2u}{dx^2}+(p-n+1)z^{-2p+2n}\frac{du}{dx}\right\}$$
$$=4z^{n-6}\frac{d^2u}{dx^2}-(4n-6)z^{n-4}\frac{du}{dx}+n(n-1)z^{n-2}u;$$

whence $(x-4x^2)\dfrac{d^2u}{dx^2}+\left\{p-n+1+(4n-6)x\right\}\dfrac{du}{dx}-n(n-1)u=0.$

7. To find the number of arrangements in which no quadruplications occur, let us write the result of Art. 5 under the form C^n_p, being the total number of arrangements of n dice, each having p faces, in which no triplications occur. Remove the dice A, B, C, and cover the face 1 of the others. Then C^{n-3}_{p-1} is the number of arrangements of those dice in which no triplications occur; and, consequently, the number in which A, B, C, and those only, show face 1. Hence $p\,C^{n-3}_{p-1}$ is the number of arrangements in which a triplication is found on the first three dice, and on those alone. Consequently the number of arrangements in which one triplet only occurs is—

$$\frac{n(n-1)(n-2)}{1.2.3}\,p\,C^{n-3}_{p-1}.$$

8. Remove the dice A, B, C, D, E, F, and cover faces 1 and 2 of the others; C^{n-6}_{p-2} is the number of arrangements in which A, B, C show face 1, and D, E, F face 2.

Hence
$$\frac{6.5.4.3.2.1}{1.2.3.1.2.3}C^{n-6}_{p-2}$$

is the number of arrangements in which A, B, C, D, E, F have faces 1 and 2 tripled; consequently

$$\frac{6.5.4.3.2.1}{(1\ 2.3)^2}\cdot\frac{p(p-1)}{1.2}C^{n-6}_{p-2}$$

is the number in which A, B, C, D, E, F form two triplets. Hence the number of arrangements in which there are two triplets, and no more, is—

$$\frac{n\,(n-1)\,\ldots\,(n-5)}{1\,.\,2\,\ldots\,6}\,.\,\frac{6\,.\,5\,\ldots\,1}{(1\,.\,2\,.\,3)^2}\,\frac{p\,(p-1)}{1\,.\,2}\,C^{n-6}_{p-2}$$

$$=\frac{n\,(n-1)\,\ldots\,(n-5)}{(1\,.\,2\,.\,3)^2}\,\frac{p\,(p-1)}{1\,.\,2}\,C^{n-6}_{p-2}.$$

9. The whole number of arrangements in which no quadruplication occurs is—

$$C^n_p+\frac{n\,(n-1)\,(n-2)}{1\,.\,2\,.\,3}\,p\,C^{n-3}_{p-1}+\frac{n\,(n-1)\,\ldots\,(n-5)}{(1\,.\,2\,.\,3)^2}\,\frac{p\,(p-1)}{1\,.\,2}\,C^{n-6}_{p-2}$$

$$+\frac{n\,(n-1)\,\ldots\,(n-8)}{(1\,.\,2\,.\,3)^3}\,\frac{p\,(p-1)\,(p-2)}{1\,.\,2\,.\,3}\,C^{n-9}_{p-3}+\,\&c.$$

10. In the same manner it may be shown, that, if the above series be represented by D^n_p, the whole number of arrangements in which no quintuplications occur is—

$$D^n_p+\frac{n\,(n-1)\,(n-2)\,(n-3)}{1\,.\,2\,.\,3\,.\,4}\,p\,D^{n-4}_{p-1}+\frac{n\,(n-1)\,\ldots\,(n-7)}{(1\,.\,2\,.\,3\,.\,4)^2}\,.\,\frac{p\,(p-1)}{1\,.\,2}\,D^{n-8}_{p-2}+\,\&c.$$

11. It is evident that the total number of arrangements of the faces is p^n. Hence the probability that no two show the same face is—

$$\frac{p\,(p-1)\,\ldots\,(p-n+1)}{p^n}.$$

XXIV. *On Solar Light, and on a Simple Photometer.* By Mungo Ponton, Esq., F.R.S.E.

(Read 4th March 1856.)

In approaching the subject of solar light, the first point is to endeavour to form an idea, not altogether indefinite, with respect to its quantity and intensity, as compared with some familiar standard of artificial flame. With this view were made, in the course of last summer, the observations now to be described.

After several trials, it was found, that the most convenient mode of procedure was first to compare a definite small surface, illuminated by solar light, with a like surface illuminated by the mere light of the sky, and then to compare the latter with a similar surface illuminated by the flame of a moderator lamp. The light of the sky thus affords a middle term between the extreme lights of the sun and the lamp, which are too diverse to be directly compared.

The first difficulty to be overcome, was that arising from the difference of colour between the flame of the lamp and the light of the sky. For this purpose, it was found necessary to employ light of only one colour; and blue light was selected, as that which could be most easily obtained pure.

Various methods having been tried, it was found that the blue rays could be obtained in sufficient purity, by taking the common blue paper used by haberdashers for packing their light goods, and steeping it in a concentrated solution of sulphate of copper, and then viewing this paper through common blue glass. This glass, it is well known, transmits only the blue rays and the extreme red; but the blue paper absorbs the extreme red, and disperses only the blue and a few yellow rays, which last are absorbed by the blue glass; so that, by this combination, only the blue rays reach the eye. In the light thus obtained, no rays save the blue could be detected by prismatic analysis.

To regulate with exactness the quantity of light admitted to the eye, a number of small slips of tea-lead were perforated with holes of various diameters, from $\frac{1}{10}$th down to $\frac{1}{100}$th of an inch. These apertures were carefully made, the rugged edges being removed, so as to present a clean circular outline. The diameter of each hole was exactly measured under the microscope, with a power of a hundred diameters.

The next point was to secure the exclusion from the eye of all extraneous light. For this purpose two pasteboard tubes were made, about eight inches long, and one inch in diameter, and lined inside with dull black paper. These were placed parallel to each other, and fastened together in such a manner that the centres of their diameters were two-and-a-half inches apart, but with the means of slightly varying this distance, so as to bring the apertures exactly opposite the pupils of the two eyes. At the end of the tubes next the eye was an aperture of about a quarter of an inch in diameter, in which was inserted a piece of blue glass. Outside of these were placed slides, into which the perforated slips of lead might be introduced, in such a manner as to prevent their apertures from touching the surface of the paper, for fear of the entrance of minute fibres. At the ends of the tubes farthest from the eyes were simple apertures of about a quarter of an inch in diameter. To each of these farthest ends was attached, at the inner edge, a piece of card, which, after projecting outwards, about an inch and a half, was bent round at a right angle, so as to face the end of the tube. These pieces of card were covered on the outside with black paper, and on the inside with the prepared blue paper before described. The lower part of the card was attached to the lower end of the tube by a similar piece of card, also lined with the blue paper; and there was thus formed, at the further end of each tube, a small rectangular box, open at the top and the outer side, and lined throughout with the blue paper. To each of these boxes were closely fitted covers, which could be put on and removed at pleasure. The cross piece connecting the two tubes was fastened to a telescope stand, so as to place the tubes horizontally at the level of the eyes. When so adjusted, the surfaces destined to receive the light were vertical.

In Plaie VIII., fig. 1, is shown a bird's-eye view of the two tubes PQ and RS; while XY is the cross piece by which they are attached, and on which they slide horizontally, so as to adjust their distance; *e e′* are the ends next the eyes, and *c c*, *c′ c′* are the cards fastened to the farther ends, on which the light is to fall. Fig. 2 is a central cross-section, showing how the tubes are connected by the cross piece, and the latter with the telescope stand T. Fig. 3 shows the two ends next the eyes, with the perforated slips of lead inserted in the slides. Fig. 4 shows the farther end of the tube, prepared to receive the sun-light, with the direction of the rays *r r′*. Fig. 5 shows the farther end of the other tube, furnished with a small screen *s* to exclude the direct rays of the sun, and leave the receiving surface illuminated by the mere light of the sky. Fig. 6 shows the end of the sun-tube, disposed for the lamp; the ring *l* being the argand burner, surrounded by a screen *s s* to confine the light. This screen was covered with black paper outside, and lined with white paper inside, so as to give the lamp-light the benefit of its reflection. There was another screen, not shown in the figure, placed over the argand, with a hole only sufficient to admit the glass chimney. By this arrangement, all light was excluded from the little box, except that of the lamp, whose flame was placed in such a position that the rays fell on the receiving surface at a horizontal angle of about 45 degrees, the surface of the flame being about two inches from the receiving surface.

When this instrument had both its receiving surfaces lighted by mere daylight, there were seen, on looking through it with both eyes, two equal round spots, one-fourth of an inch in diameter, of a very pure blue colour. If the apertures next the eyes were equal, these spots appeared of exactly the same tint and intensity; but if one of the apertures was a very little smaller than the other, the spot, viewed through the smaller aperture, appeared of a sensibly darker shade, in so much as to impress the eye with an idea of difference of colour. This peculiarity greatly aids the eye in judging when the two spots are of exactly equal brightness. It is, of course, necessary to take care that both apertures are smaller than the pupil of the eye.

In order to determine how far the results to be obtained by this instrument might be found to agree with those which may be obtained from the method of equal shadows, the instrument was first employed to compare the light of the moderator lamp with that of a wax candle (a short 6); and, after repeated trials, it was found that the results of the comparison, when made with this instrument, exactly agreed with those obtained by the method of equal shadows, the light of the lamp proving to be 3½ times that of the candle. Indeed it is, if anything, easier to judge of the perfect equality in brightness of the two blue spots, than of the equal darkness of two shadows.

Several preliminary trials were next made, with a view to obtain a series of approximations to the relative sizes of the apertures to be employed. In making these observations, the observer should sit in an easy, half-reclining posture, with his back to the sun, so as to have the receiving surface exactly opposite to that luminary; his elbows should be rested steadily on the table; and his hands placed at the sides of his eyes, to screen them from extraneous light. Before looking into the instrument, the eyes should be closed for a little time, to render them more sensitive to the feeble blue rays; and care must be taken to have the two images simultaneously visible without effort, so as to admit of their exact comparison. For a good observation, a bright, cloudless, and perfectly calm day, should be chosen; and the sun should be at an altitude of about 45°, so as to give the mean brilliancy of sunshine.

Thursday the 10th of August 1855 having proved entirely suited for the purpose, advantage was taken of it, to make the decisive observations, which were commenced about 11 A.M., and occupied between 15 and 20 minutes.

The diameter of the aperture fitted to the sky-tube was 0·083 inch. The sun-tube was tried first with an aperture of 0·015, through which the sun-lit surface appeared a little brighter than that lighted by the sky. It was next tried with an aperture of 0·01375, and then the sky-lit surface appeared the brighter of the two. Lastly, there was applied to the sun-tube an aperture of 0·014375, and then no appreciable difference could be detected by the most steady gaze. These three apertures were changed several times, and always with the same results.

The little box at the end of the sun-tube had now its cover put on, so as to leave open only its right-hand side, to which the lamp was approached, and disposed in the manner before described. The aperture applied to this tube was now 0·1, and, after trying several approximate apertures for the sky-tube, one of 0·0275 was ultimately fixed upon, as that which rendered the two images of exactly equal brightness.

These observations were subsequently repeated, with the same results; and, on each occasion, the apertures were, after the observations, examined under the microscope and ascertained to be clear. These results may therefore be regarded as a fair approximation to the truth.

The brightness of the images being inversely as the areas of the apertures, it follows from the first observation, that a small surface illuminated by the direct rays of the sun at an altitude of 45° is 33·6 times brighter than a similar surface illuminated by diffuse day-light; while, from the second observation, it follows that a like surface illuminated by the flame of a moderator lamp, at the distance of 2 inches, and placed obliquely so that the rays might fall as nearly as possible at a horizontal angle of 45°, is 13·2 times less bright than a similar surface illuminated by diffuse day-light. Hence, the same surface when lighted by the sun is 444 times brighter than when lighted by the lamp, under the above circumstances, the blue rays only being used in each case. This exclusion of all but the blue rays, is somewhat adverse to the artificial light, which has an excess of red and yellow rays, beyond what is required for the composition of white light; but the blue rays may be held to indicate the proportion of white light, contained in the artificial flame.

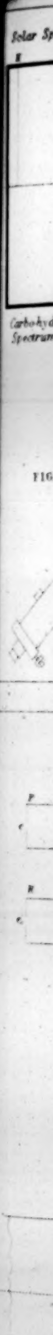
As, from the preliminary observation, it was found that the moderator lamp employed was 3·5 times brighter than a wax candle (short 6 in the lb.), it follows, that a small surface, illuminated by mean sunshine, is 1554, or say 1560 times brighter than is the same surface when lit by such a wax candle placed at 2 inches from it, in an oblique direction.

Now, it is found not difficult to raise the electric light to such a pitch of intensity as to afford a light equal to that of 520 wax candles; so that, if the moderator lamp were replaced by three such electric lights, the surface would be equally bright as when illuminated by mean sunshine.

To form a conception, therefore, of the quantity and intensity of the light emanating from the sun, when it reaches a distance of 95 millions of miles from his centre, we may imagine the surface of a sphere, having that distance for its radius, to be covered all over with a very thin film, say $\frac{1}{1000}$th of an inch in thickness, having a brightness equal to that of an electric light of the above-mentioned intensity, and that behind this there are two similar films of equal brilliancy, the three forming a thin stratum, say $\frac{1}{40}$th of an inch in thickness; then such a stratum would represent the brilliancy of the sun's light at the earth's orbit.

Now, let us imagine this stratum to be transferred to the surface of the sun. It would there be spread over 46,275 times less area; consequently, its thickness would be increased that number of times, and would therefore amount to about 1132 inches, or about 94 feet, embracing 138,825 layers of flame, equal in brightness to an electric light of the above-mentioned intensity, and it would at its outer surface possess a brilliancy equal to that of the surface of the sun.

It is evident, however, that there might be a very considerable addition made to the thickness of such a stratum, without affecting, in any appreciable degree, its proportion to the planetary distances. If then, the thickness of the stratum were increased 520 times, making it 49,000 feet, then it might embrace 72,000,000 layers, each of them having an individual brilliancy not greater than that of a wax candle. The real thickness of the stratum in which the luminous property of the sun resides, may be very considerably greater than the above estimate, which is somewhat over 9 miles; and the luminosity of each individual film composing the stratum may be very considerably less, without affecting the general result.


Solar Sp
Carbohyd
Spectrum
FIG

Mr. Swan's observations of the Carbohydrogen Spectrum

FIG. 1

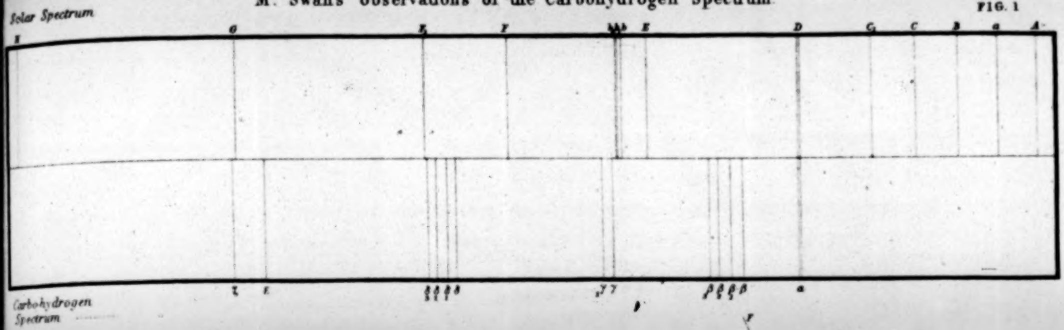


FIG. 2

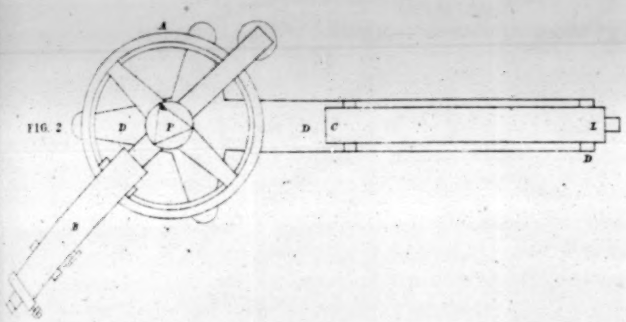


FIG. 3

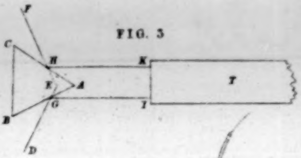
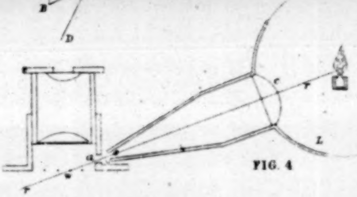


FIG. 4



Mr. PONTON'S PHOTOMETER.

FIG. 1 ½ size

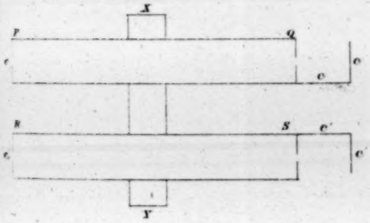


FIG. 2 ½ size

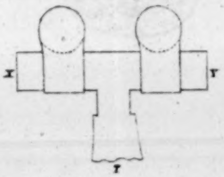


FIG. 3

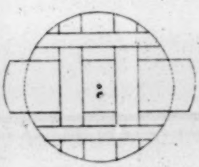


FIG. 3

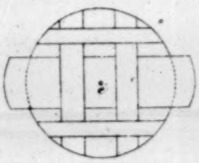


FIG. 6

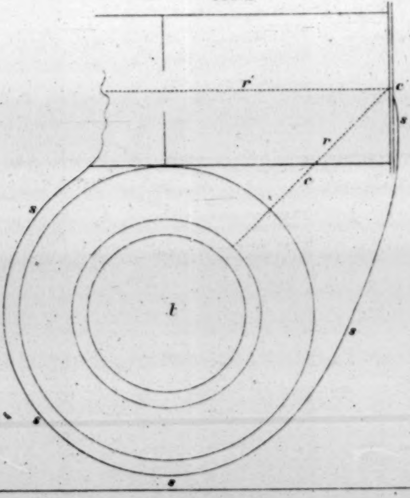


FIG. 4

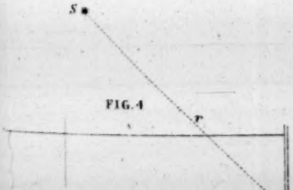
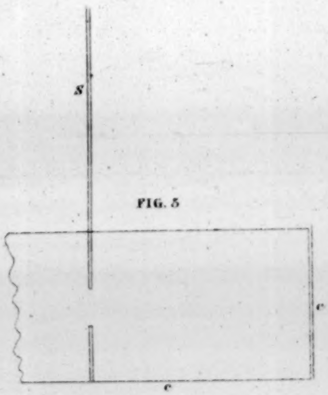


FIG. 5



W. & A. K. Johnston, Edinburgh.

BRITISH MUSEUM

XXV.—*On the Possibility of combining two or more Probabilities of the same Event, so as to form one Definite Probability.* By the Right Rev. Bishop TERROT.

(Read 17th March 1856.)

(1.) The inquiry which, with its results, I propose to lay before the Society, was suggested by the following passage in the very popular Treatise on Logic by Dr WHATELY, now Archbishop of Dublin.

"As in the case of two probable premises, the conclusion is not established except upon the supposition of their being *both* true, so in the case of two (and the like holds good with any number) distinct and independent indications of the truth of some proposition, unless *both* of them *fail*, the proposition must be true: we therefore multiply together the fractions indicating the probability of the failure of each—the chances against it—and the result being the total chances against the establishment of the conclusion by these arguments, this fraction being deducted from unity, the remainder gives the probability *for* it.

"*E. g.* A certain book is conjectured to be by such and such an author, partly, 1*st*, from its resemblance in style to his known works; partly, 2*d*, from its being attributed to him by some one likely to be pretty well informed. Let the probability of the conclusion, as deduced from one of these arguments by itself, be supposed $\frac{2}{5}$, and in the other case $\frac{3}{7}$; then the *opposite* probabilities will be respectively $\frac{3}{5}$ and $\frac{4}{7}$, which multiplied together give $\frac{12}{35}$ as the probability against the conclusion; *i. e.*, the chance that the work may *not* be his, notwithstanding the reasons for believing that it is; and, consequently, the probability in *favour* of the conclusion will be $\frac{23}{35}$, or nearly $\frac{2}{3}$" (WHATELY'S *Logic*, 8th Ed., p. 211.)

(2.) Now, this reasoning appears to me erroneous, because it can be so applied as to bring out two inconsistent conclusions. It must be observed, that there is no such generic difference between the chances *for* and *against* the truth of a proposition, as can require or justify any difference in the laws and methods applied to them. A negative can always be turned into an affirmative by a change of verbal expression, without any change of meaning. Thus the chance of not hitting a mark is the same as the chance of missing it. The chance of a life not falling before sixty, is the chance of its continuance up to sixty. The chance that A was not the author of the book, is the chance that some one else was the author.

Let us then take as the proposition whose probability is to be found, the negative —he did not write it—the partial probabiilties *for* which are by the data $\frac{3}{5}$ and $\frac{4}{7}$.

The opposite probabilities are now $\frac{2}{5}$ and $\frac{3}{7}$, and their product is $\frac{6}{35}$, the probability *against* the conclusion whose probability we are now seeking. Consequently, $1-\frac{6}{35}=\frac{29}{35}$ is the probability *for* our conclusion, namely, that he did not write the book. But by the former calculation, the probability of the same conclusion was found to be $\frac{12}{35}$: and, as these incompatible results follow from the same principle and method, the principle and method must be erroneous.

(3.) The only mathematical attempt at the solution of this problem which I have met with, is at section 15 of the Article Probability, in the *Encyclopœdia Metropolitana.* It is given there as follows:—

" It is an even chance that A is B, and the same that B is C; and, therefore, 1 to 3 on these grounds alone, that A is C. But other considerations of themselves give an even chance that A is C. What is the resulting degree of evidence (or the probability) that A is C?" There is a previous solution which I omit, and then the passage proceeds as follows:—" Let us now treat the preceding question as having two contingencies, the compound argument 1 to 3 *for*, and the independent evidence an even chance. We have, therefore, four possible cases.

	PROB. A is C.
" Argument and Evidence both true, . . .	$\frac{1}{4}\times\frac{1}{2}=\frac{1}{8}$
Argument false, Evidence true, . . .	$\frac{3}{4}\times\frac{1}{2}=\frac{3}{8}$
Argument true, Evidence false, . . .	$\frac{1}{4}\times\frac{1}{2}=\frac{1}{8}$
Argument and Evidence both false, . .	$=0$

"The sum of these is $\frac{5}{8}$ as before (for the resulting probability that A is C). The above generalized is as follows:—Let a and $(1-a)$ be the probabilities for and against the argument (the conclusion from the argument); and ϵ and $(1-\epsilon)$ be the probabilities from any other source. Then the chance that both are wrong is $(1-a)\,.\,(1-\epsilon)$, and of the contradictory, namely, that (A is C) follows from the one or the other, is $1-(1-a)\,.\,(1-\epsilon)=a+\epsilon-a\,\epsilon$."

This is the formula adopted by WHATELY; and it is open to the same objection, namely, that by applying it we can arrive at two contradictory conclusions. But, further than this, what is the meaning of *Argument true, Evidence true?* The argument and the evidence are here treated as two independent events having respectively the probabilities of $\frac{1}{4}$ and $\frac{1}{2}$; and their coincidence is represented by $\frac{1}{8}$. But nothing corresponding to this goes forward in the mind. The argument merely affords the information, that for every reason for believing that A is C, there are three equivalent reasons for believing that A is not C. This information we are supposed to believe absolutely; there is no question as to the probability of its truth, or the possibility of its falsehood. The only matter in question is whether A is C, or is not C.

The falsity of the expression $a+\epsilon-a\,\epsilon$ will be evident, if we give to a and ϵ the values $\frac{2}{7}$ and $\frac{3}{7}$. Then $a+\epsilon-a\,\epsilon=\frac{2}{7}+\frac{3}{7}-\frac{6}{49}=\frac{1}{2}+\frac{10}{49}$. That is to say, while each of the independent probabilities is less than $\frac{1}{2}$, and, therefore, in favour of the negative, their compound force is much above $\frac{1}{2}$; and, therefore, in favour of the affirmative. If then we found from internal evidence and external evidence severally, that the chances were against the truth of the proposition A is C, we ought to conclude from their united force, that the chances are in favour of the proposition. But the human mind is incapable of coming to such a conclusion.

It may be well to notice in passing, that the problem under consideration is altogether different from that of finding the compound force of two identical assertions made by two witnesses, whose veracity, that is, the probability of their speaking truth, is expressed by a and ϵ. In that problem, we possess among the data the fact, that each witness *makes the same assertion.* But in the problem we have been considering, there is no such *assertion.* Neither the argument nor the evidence assert or deny that A is C. What they give as data, is merely that the reasons for believing that A is C, are in a given ratio to those for believing that A is not C. And as the data of the two problems are of totally different character, the methods to be applied must of course be different. I have mentioned this, because some good mathematicians whom I have consulted, were at first disposed to consider $\frac{a\epsilon}{a\epsilon+\overline{1-a}\,.\,\overline{1-\epsilon}}$ as the proper expression for the conjoined force of the argument and evidence.

(4.) Let us now consider the Problem under the following form. *A, whose veracity is undoubted, states that, from his knowledge of the facts of the case, the probability of the event* E *is* $\frac{p}{q}$. *B, under the same conditions, states, that it is* $\frac{r}{s}$. *Supposing the facts known by each to be altogether distinct, what is the proper measure of the expectation formed in a third mind by these two statements?*

(5.) Before attempting to show how a solution of the problem ought to be sought, it may be well to observe, that the mind cannot admit two probabilities of the same event as co-existent probabilities. Thus, if A tells me that the probability of rain to-morrow is $\frac{2}{5}$, and B that it is $\frac{4}{7}$, I cannot admit both of these as probabilities; for that would be equivalent to believing on the authority of A, that it is *less* likely to rain than not, and at the same time to believe on the authority of B, that it is *more* likely to rain than not.

What really takes place is this. The two fractions are received as indications of the effects reasonably produced upon the minds of the informants, by the knowledge of certain facts which they have not communicated to us. The fractions which they give are admitted as true exponents of the results of their respective partial knowledge, no doubt resting either upon their veracity, or upon the accuracy of their inferences. We admit, that each states the probability as he ought, under *his* circumstances: and the question is, how ought we to state it under *our* circumstances, knowing as we do something *more*, and also something *less* than either of our informants.

(6.) In attempting to answer this question, I shall have recourse to the ordinary illustration of an urn and balls. Let us suppose that A has seen p white and $q-p$ black balls introduced into an urn, which he believes to have been previously empty. He properly infers that the probability of drawing a white is $\frac{p}{q}$. B, under the same circumstances, has seen r white, and $s-r$ black balls introduced, and infers that the probability of drawing a white is $\frac{r}{s}$. If they communicate to each other only their *inferences*, there is an apparent contradiction, and no combination or agreement can take place. But if they communicate the *facts* from which the inferences were deduced, then each knows that the urn contains $p+r$ white, and $q+s-p-r$ black balls, and agree in making the probability of drawing a white $\frac{p+r}{q+s}$. If the number of balls whose introduction has been seen by the two observers be equal, then $\frac{p+r}{q+s}=\frac{p+r}{2q}=\frac{1}{2}\left(\frac{p}{q}+\frac{r}{q}\right)=\frac{1}{2}$, the sum of the several probabilities.

It may be observed that $\frac{p+r}{q+s}$, as the expression of the combined probabilities $\frac{p}{r}$ and $\frac{q}{s}$, is not exposed to the objection of admitting contradictory results, for, if we take the negative as the conclusion whose probability is to be found, then A gives for the probability of the conclusion $1-\frac{p}{q}=\frac{q-p}{q}$, while B gives $1-\frac{r}{s}=\frac{s-r}{s}$, therefore the combined probability against the event is $\frac{q-p+s-r}{q+s}$. But the combined probability for the event was $\frac{p+r}{q+s}$, and $\frac{p+r}{q+s}+\frac{q-p+s-r}{q+s}=\frac{q+s}{q+s}=1$, as it ought to be.

(7.) But what we have to consider, is the impression made upon the mind of a third person, who is informed by A that, from his observation, the probability of drawing a white is $\frac{p}{q}$, and by B that, from his observation, it is $\frac{r}{s}$, and to whom no farther information is given, except that the observations were totally distinct. Now, as these data give only the ratio of white to black balls at each introduction, there may have been, in the first, p white and $q-p$ black, or there may have been np white and $nq-np$ black, where n is any whole number from one to infinity. In like manner, the second may have consisted of nr white and $ns-nr$ black, where n is any number from one to infinity. Any one assumed state of the first introduction may have co-existed with any assumed state of the second; and thus assuming that the first contained p white and $q-p$ black, we have the infinite series of probabilities,

$$\frac{p+r}{q+s-p-r}, \frac{p+2r}{q+2s-p-2r} \dots\dots \frac{p+nr}{q+ns-p-nr}.$$

Again, assuming that the first contained $2p$ white, and $2q-2p$ black, we have

$$\frac{2p+r}{2q+s-2p-r}, \frac{2p+2r}{2q+2s-2p-2r} \dots\dots \frac{2p+nr}{2q+ns-2p-nr},$$

and so on *ad infinitum*.

This infinite series of infinite series I cannot sum. If they can be summed, then their sum divided by the infinite of the second order n^2, is the probability required.

In no case, except when $\frac{p}{q}=\frac{r}{s}$, so far as I see, can the sum of their sums, or the whole probability, be determinately expressed. When $\frac{p}{q}=\frac{r}{s}$, the fractions being in their lowest terms, $p=r$ and $q=s$. The two pieces of information are then identical; the same information is given by both observers; and the information, unaffected by the repetition, is absolutely received by the third party: and this is the result, if, in the foregoing series, we substitute p for r and q for s.

(8.) If we revert to the expression (3) given in the *Encyclopædia Metropolitana*, where the separate probabilities are a and ϵ, and their conjoint force is stated to be $a+\epsilon-a\epsilon$, it would follow that the effect produced by two observers making the same statement as to the probability of an event should be twice the asserted probability *minus* its square. Now, in the case of a repetition of the same probability by two observers, it must, I think, be allowed that my result is conformable to that of which we are all conscious. If, for example, the Northampton and the Carlisle Tables both give $\frac{1}{2}$ as the probability that a man of thirty will live to the age of fifty, and are both implicitly believed, we believe that there is an even chance of his living to fifty, and not, as would follow from the expression given in the Encyclopædia, that the chances are three to one in his favour.

(9.) It perhaps deserves to be noticed, that when a second series of observations or experiments is added to one previously admitted, the probability is not increased by the mere preponderance of favourable over unfavourable cases in the second series. To increase the probability, the ratio of favourable to unfavourable cases must be greater in the second series than in the first. For the first received probability is $\frac{p}{q}$, and the composite is $\frac{p+r}{q+s}$. (6.)

$$\text{Now } \frac{p+r}{q+s} > \frac{p}{q}, \text{ when } r\,q > s\,p, \text{ or } r\,q - r\,p > s\,p - r\,p,$$

$$\text{or} \quad r\,(q-p) > p\,.\,(s-r), \text{ or } \frac{r}{s-r} > \frac{p}{q-p}\,.$$

When the ratios only are given, any conceivable case of the grounds upon which the probabilities are given may be represented by $m\,p$, $m\,q$, $n\,r$, and $n\,s$. Hence the original probability is $\frac{m\,p}{m\,q}$, the composite is $\frac{m\,p+n\,r}{m\,q+n\,s}$. and this is greater than $\frac{m\,p}{m\,q}$. when

$$m^2\,p\,q + m\,n\,q\,r > m^2\,p\,q + m\,n\,p\,s$$

$$\text{or } r\,q > p\,s, \text{ as before.}$$

(10.) Valid objections may, I think, be made to the last paragraph of the section in the Encyclopædia already referred to. As this is not long, I quote it entire. "The following theorem will be readily admitted on its own evidence. *If any assertion appear neither likely nor unlikely in itself, then any logical argument in its favour, however weak the premises, makes it in some degree more likely than not.* In the manner in which writers on Logic apply the calculus of probabilities, this is never the consequence of their suppositions. For what we have called a is their resulting probability of the argument. Suppose, for instance, a writer on logic presumed that the argument from analogy gave $\frac{3}{10}$ to the probability that there is vegetation in the planets, which must be regarded as a thing neither likely nor unlikely in regard of evidence from any other source, he would take $\frac{3}{10}$ to be the probability of this result, that is, *less after an argument in its favour than it was before.* We substitute $\frac{1}{2} + \frac{1}{2} \cdot \frac{3}{10} = \frac{13}{20}$."

This numerical equation is the value of the expression $(a + \epsilon - a\,\epsilon)$, when $a = \frac{1}{2}\ \epsilon = \frac{3}{10}$. I have already shown that this expression does not truly represent the composite force of the two probabilities a and ϵ. But farther than this, the argument from analogy, giving $\frac{3}{10}$ as the probability of the affirmative, is an argument, not *in favour of*, but *against* the proposition that there is vegetation in the planets. It implies that for every three reasons for believing that there *is*, there are seven for believing that there *is not;* and, consequently, the effect of the argument ought to be to diminish our disposition to believe the proposition, or, in other words, to diminish its probability.

(11.) But it may be worth while to examine whether the fraction $\frac{1}{2}$ be, after all, a true available expression for the probability of an event, which is neither likely nor unlikely to happen, or to have happened, there being no evidence, no reasons for belief, either for or against it.

Probability, as Mr BOOLE in his *Laws of Thought*, properly defines it, is "Expectation founded upon partial knowledge." Events, therefore, of which we possess *complete* knowledge, and events of which we possess *no* knowledge, are equally, by the terms of the definition, excluded from the class of probable events, that is to say, of events to which the calculus of probabilities can be applied. If we are *certain* that an event has happened, we totally neglect and are unaffected by any subsequent information, which, but for that certainty, would have given to the event a definite probability expressed by a proper fraction; and never think of looking for a form by which to combine such fraction with the unit expressing the certainty. If, again, we derive from experience or observation a definite probability of any event, such, for example, as the probability for drawing a white ball from an urn, whose contents are given; namely, the fraction whose numerator is the number of white balls, and its denominator the total number of balls contained, we never think of combining this with the $\frac{1}{2}$ which is assumed as the probability when nothing whatever was known, except that the ball drawn must be either white or not white. *Complete* knowledge comprehends all previous partial knowledge; and, therefore, all fractional expressions for probability derived from the latter, are virtually contained in the unit, which is the expression adopted for the certainty produced by the former. On the other hand, *partial* knowledge destroys total ignorance, and any inference that may be drawn from it. It comprehends the hypothesis that the event may, and that it may not happen, with a definite probability to each, which do not supplement but supersede the probabilities of $\frac{1}{2}$ previously assumed for each. I cannot conclude without suggesting a doubt, whether $\frac{1}{2}$ be at any time the proper expression for the probability of an event which is "neither likely nor unlikely in regard of evidence." It seems more analogous with the practice in other cases to express such probability by the indefinite fraction $\frac{0}{0}$. If this expression be applied to either of the probabilities constituting the compound probability $\frac{p+r}{q+s}$, the compound probability will be reduced to the remaining simple probability, for $\frac{0+r}{0+s}=\frac{r}{s}$. And this agrees with the necessary action of the mind, which takes no note of its original ignorance, after it has arrived at a definite probability from partial knowledge.

(12.) Hitherto, I have been speaking of the combined result of two probabilities of the same event, derived from distinct sources of partial knowledge; and I have shown that to obtain a definite result, the mere ratio in such case is insufficient, and that the actual number of favourable and unfavourable cases in each of the data is requisite.

But when the given probabilities are of different events, and the quæsitum is the probability of their joint occurrence, the ratio alone is sufficient, because as factors $\frac{mp}{mq}$ and $\frac{np}{nq}$ give the same results.

(13.) To sum up the propositions proved in the foregoing paper:—

1. If the *ratio only* of equally probable cases, in two or more probabilities for the same event be given, no definite probability can be derived from their composition. (7.)

2. If the two given probabilities $\frac{p}{q}$, $\frac{r}{s}$, indicate not merely the *ratio*, but also the *actual number* of favourable and unfavourable hypotheses or cases, their conjoined force is properly expressed by $\frac{p+r}{q+s}$. (6.)

3. Under both of these conditions, the second given probability increases or diminishes the force of the first, according as the fraction expressing the second is greater or less than that expressing the first. When the ratios only are given, then the extent of increase or diminution is indefinite. When the actual numbers are given, it is definite. (9.)

4. The *a priori* probability derived from absolute ignorance has no effect upon the force of a subsequently admitted probability. (11, 12.)

XXVI.—*Researches on Chinoline and its Homologues.* By C. Greville Williams,

Assistant to Dr Anderson, University of Glasgow.

(Read 7th April 1856.)

Twenty-two years have now elapsed, since Runge first published his remarkable experiments on coal naphtha,* and it would, perhaps, be difficult to instance any chemical investigation which has formed the point of departure of a greater number of researches. When we consider the vast quantity of bodies which have, first and last, been obtained from coal-tar, it might appear that little more remained to be done,—that the mine was exhausted,—but so far from this being the case, the discovery of one substance has only served to pave the way for the isolation of others.

Among the bodies examined by Runge, there was one which apparently possessed comparatively few features of interest; indeed its very name (the first syllable derived from λευκός) was intended to express its supposed inability to produce coloured reactions, a feature which, in the chemistry of the time, militated greatly against its claims to notice. I have used the expression "supposed inability," because I shall show further on, that this substance is capable, under certain conditions, of affording extremely brilliant colorations. Eventually, Gerhardt,† by acting on quinine, cinchonine, and strychnine, with hydrate of potash, obtained the same body. The first chemist who succeeded in procuring any of its compounds in a state of tolerable purity was Hofmann, whose analysis of the platinum salt is very nearly exact. But, at the time of that analysis, he was of opinion that the products obtained from coal and chinoline were essentially different, an opinion which he subsequently retracted. In the mean time, the alkaloid, as obtained from cinchonine was examined by Bromeis‡ and Laurent,§ their results, however, not elucidating the composition of the basic fluid obtained in the manner alluded to.

Some time since, I undertook the examination of the bases produced by destructive distillation of the bituminous shale of Dorsetshire, and found them to be identical with those from bone-oil.‖ I now began to see the great probability that all processes of destructive distillation of nitrogenous matter at very elevated temperatures would result in the formation of the same classes of alkaloids, and subsequent researches* have only tended to confirm this view. In a little paper, "On some of the basic constituents of Coal-Naphtha, and on Chrysène,"† I have given a table, showing the extraordinary similarity of the basic products derived by dry distillation from Dippel's oil, coal, the Dorset shale, and cinchonine. The last of these researches was undertaken in the endeavour to throw light upon the discrepancies in the results of the chemists who had previously examined chinoline, the experiments being embodied in a paper which appeared in the Transactions of the Society last year. In that communication,‡ I showed that the fluid usually known as chinoline, and supposed to have the formula $C_{18}H_7N$, had, in fact, a very complex constitution, and contained in addition to that base, six others. As my chief object at that time, was to demonstrate the real nature of the decomposition which cinchonine undergoes at an elevated temperature in the presence of alkalies, I did not make a minute examination of the chinoline itself, as I conceived it to be sufficient for the purposes of that investigation to show that a base of the formula $C_{18}H_7N$ did really exist in the fluid. This fact was by no means a matter of course, for the analyses of the chinoline from cinchonine previously published were so conflicting, that it was a difficult matter to derive a formula from them. Hofmann's analyses were made upon a product from coal-tar, and the formula he gave as the expression of his results, was $C_{18}H_8N$. But as an even number of atoms of hydrogen in a body containing an equivalent of nitrogen, was incompatible with views now almost universally received of the constitution of organic bodies, $C_{18}H_7N$ was taken by most chemists as the true formula of the base from coal-tar. But the wide differences in the analyses of the chinoline obtained by distilling cinchonine with potash, induced Gerhardt § to express doubts, as to whether $C_{18}H_7N$, or $C_{20}H_9N$ was the correct formula, although he appears to lean towards the latter, for he places it at the head of the section, but, nevertheless, shows that the formula is open to doubt, by annexing a note of interrogation to it. I have shown the cause of the variable nature of the results obtained by other experimenters, and have proved the existence of a homologous series, of which, until I commenced this investigation, only one member was known.

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* Poggend. Annal., Bd. xxxi., p. 65 und 513; und Bd. xxxii., p. 308 und 328.

† Revue Scientif., x., 186. Compt. Rend. des Trav. de Chim. 1845, p. 30.

‡ Liebig's Annal., Bd. lii., p. 130; and Ann. der Chem. u. Pharm. lii., 130.

§ Ann. de Chim. et de Phys. [3] xxx., 368.

‖ Quart. Jour. Chem. Soc. Lond., July 1854.

Many circumstances conspire to render a detailed examination of chinoline a problem of interest, for, perhaps, no other body, known for an equal length of time, and investigated by so many hands, is so erroneously described in the manuals of organic chemistry. In fact, there are few things stated regarding it, that are not more or less incorrect.

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* I take this opportunity of expressing my sense of Dr Anderson's kindness, in permitting me to make use of his laboratory and apparatus, during my endeavours to realize this idea.

† Chem. Gazette, Nov. 1, 1855; and Edin. Phil. Jour., Oct. 1855.

‡ Trans. Roy. Soc. Edin., vol. xxi., part ii.

§ Traité de Chimie Organique, troisième partie, p. 148.

Chinoline has, however, been invested with an artificial interest, from a supposed intimate connexion between it and quinine, and an equally supposititious parallelism between the action of heat upon the last named alkaloid, and upon the hydrated oxide of tetramethylammonium, while the real points of attraction which it possesses have been neglected, or supposed not to exist.

The first incorrect idea of the connexion between chinoline and quinine, may be very briefly disposed of. It was founded upon the supposition that chinoline was the sole product of the action of hydrate of potash, at a high temperature, upon quinine. In this manner, it was easy to construct an equation by which it was made to appear, that quinine, *minus* a certain number of equivalents of carbon, hydrogen, and oxygen, yielded chinoline.

Another supposed connexion between the two alkaloids was a very beautiful one, and one that, at the time when $C_{18}\,H_8\,N$ was the received formula for chinoline, could scarcely have failed to suggest itself to the eminent chemist, whose particular train of research led him to examine the action of iodide of methyl upon the natural and artificial alkaloids. It is well known, that iodide of tetrethylammonium, by treatment with oxide of silver, yields hydrated oxide of that base, which is rendered obvious, by a glance at the following equation :—

$$N\left\{\begin{matrix}C_4\,H_5\\C_4\,H_5\\C_4\,H_5\\C_4\,H_5\end{matrix}\right\}I+Ag\,O+HO=N\left\{\begin{matrix}C_4\,H_5\\C_4\,H_5\\C_4\,H_5\\C_4\,H_5\end{matrix}\right\}O,HO+Ag\,I;$$

and if we follow out the same equation, substituting iodide of methyl-chinoline-ammonium,* for iodide of tetrethylammonium, we find that at first sight,

$$\underbrace{C_{20}\,H_{11}\,N,\,I}_{\text{Iodide of Methyl-chinolineammonium.}}+Ag\,O+HO=\underbrace{C_{20}\,H_{12}\,NO_2}_{\text{Quinine.}}+Ag\,I$$

appears a reaction likely to take place ;† unfortunately, however, there are two reasons why it is impossible, the first being, that iodide of methyl-chinoline-ammonium is represented by $C_{20}\,H_{10}\,N,\,I$, instead of $C_{20}\,H_{11}\,N,\,I$; and the other, that the action of oxide of silver upon the methyl and ethyl compounds of the nitryl bases of this class is more complex than would be supposed from the first equation, and the known success of the reaction with the iodides of the ammonium bases derived from the alcohol radicals alone.

So much has been said about the artificial formation of quinine from the leukol of coal-tar, that I have appended a few reasons for concluding that it is impossible by any analogous process to that previously described. Quinine, according to Strecker's experiments,[*] takes up only one equivalent of the alcohol radicals, and is therefore concluded with safety to be a nitryl base. Chinoline affords still more complete evidence of belonging to the same class, for not only is it incapable of taking up more than one equivalent, but, by the operation, it becomes converted into a fixed alkaloid. Now, any process for making artificial quinine by means of the reactions mentioned above, would result in the formation of an ammonium base, which must of necessity have a totally different constitution to quinine. It may be worth while, for a moment, to glance at the formidable difficulties by which the artificial formation of such a base as quinine is surrounded. In the first place, in the present state of our information, it appears to consist of three radicals, united and having one equivalent of nitrogen and two of oxygen attached. Now, to acquire a knowledge of the constitution of these three radicals (one of which, in all probability, is oxidized) is a problem involving a new mode of research, the key to which appears, for the present, to be hidden. And even supposing the three radicals known, they have to be formed; and then to combine them with the addition of an equivalent of nitrogen, without destroying the group, presents a task of no ordinary difficulty.

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* Supposing for the moment the old formula for chinoline ($C_{18}\,H_8\,N$) to be correct.

† I have vainly searched through the Chemical Journals for any paper by Dr Hofmann, tending to show the real nature of the action of oxide of silver upon iodide of methylchinoline. This has led me to make the experiments detailed at page 392.

I should not have entered upon this branch of the subject, had it not been for the manner in which the possibility of the formation of quinine, by the method above alluded to, has been accepted as a reality, which is the more remarkable from the manner in which Hofmann cautioned chemists against placing too much reliance upon the success of the process.

As it is my wish to correct, as far as my information will permit me, the erroneous views which have been formed of the relations between chinoline and quinine, I return to the supposed similarity between the action of heat upon quinine and the hydrated oxide of tetramethylammonium. This part of the subject is the more interesting, as it appears to have formed one of the links in the chain of reasoning, which led to a belief in the possibility of converting leukol into quinine, by the successive actions of iodide of methyl and oxide of silver. As the fixed base, hydrated oxide of tetramethylammonium, by heating yields trimethylamine, the difference being $C_2 H_4 O_2$, so quinine, less $C_2 H_4 O_2$ yields the old formula of leukol[†] thus,—

$$\underbrace{C_8 H_{13} NO_2}_{\text{Hydrated Oxide of Tetramethyl-ammonium.}} - C_2 H_4 O_2 = \underbrace{C_6 H_9 N}_{\text{Trimethyl-amine.}}$$

$$\underbrace{C_{20} H_{12} NO_2}_{\text{Quinine.}} - C_2 H_4 O_2 = \underbrace{C_{18} H_8 N}_{\text{Leukol.}}$$

The identity in kind of the above equations presupposes two conditions, neither of which exist, the first being the correctness of the old formula for chinoline (or leukol), the second, its being the sole basic product in the distillate, from the cinchona alkaloids.

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[*] Researches in Organic Chemistry, by Adolph Strecker. Compt. Rend. xxxi. 49. Chem. Soc., Quart. Jour. 1854, vol. vii., p. 278.

[†] Quart. Jour. Chem. Soc., vol. iv., p. 328.

The next prevalent error with regard to chinoline is, that its salts have less tendency to crystallize than the generality of nitryl bases; whereas, in fact, the reverse (with some exceptions) is the truth. I have seldom seen salts more easy to crystallize than the nitrate, oxalate, and bichromate of chinoline, while its double salts, with platinum, gold, palladium, uranium, and cadmium, are beautiful substances, the same may be said of the iodides of the methyl, ethyl, and amyl compounds. The erroneous idea alluded to arose from previous experimenters, working on an impure substance.

The only means for determining the constitution of chinoline up to the present time, has been Dr HOFMANN'S analysis of the platinum salt, from a base extracted from coal-tar; for the combustions of the base itself yet made, are very unsatisfactory. Annexed are the results as yet obtained.*

	HOFMANN.			BROMEIS.		$C_{18}H_7N$	$C_{20}H_9N$
Carbon, . . .	82·67	82·88	82·34	82·74	82·78	83·70	83·91
Hydrogen, . .	6·56	6·25	6·10	6·11	5·88	5·41	6·29
Nitrogen, . .	11·28	...	...	...	...	10·89	9·80
						100·00	100·00

A glance at the above numbers shows that no conclusion can be drawn from them; and when it is considered, that chinoline and lepidine only differ by ·21 in their percentage of carbon, it becomes evident, that careful analyses of the salts of these bases are the only means by which their history and composition can be rendered certain. The platinum salt of chinoline possesses characters which render it peculiarly well adapted for this purpose, inasmuch as it differs totally from the corresponding compound from the Dippel and aniline series, in its great insolubility. I therefore selected this compound as a means of ascertaining the purity of the various fractions obtained in the course of the investigation, and which were intended for conversion into the various salts described further on; sometimes I was contented with merely a platinum determination, at others, I ascertained by combustion with chromate of lead, the percentage of carbon and hydrogen, and in this manner, the analyses quoted below were obtained. In my former paper, I gave the result of three combustions of the platinum salt of chinoline, and three platinum determinations; the salts analysed were obtained from fractions boiling at a somewhat lower temperature than those the details of the analyses of which are given below. The following analyses were made with salts obtained from fractions boiling about 460°, which is, probably, very nearly the boiling point of chinoline.

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* GERHARDT, Traité, troisième partie, p. 149.

I.	8·900	grains of	platinum-salt of chinoline from fractions boiling between 450° and 460° F. gave
	10·559	...	carbonic acid and
	2·191	...	water, and
	6·235	...	platinum salt of chinoline, gave
	1·827	...	platinum.
II.	6·057	...	platinum-salt of chinoline gave
	1·772	...	platinum.
III.	5·907	...	platinum-salt of chinoline gave
	1·731	...	platinum.

or, per cent.—

	I.	II.	III.	Mean.
Carbon, . .	32·36	...	...	32·36
Hydrogen, . .	2·74	...	...	2·74
Nitrogen, . .	...	...	...	...
Chlorine, . .	...	...	...	...
Platinum, . .	29·30	29·26	29·30	29·29

In the following table, the result of all my analyses (including those in the former paper) is compared with the numbers required by theory; the analysis just quoted being the fourth in the series :—

	I.	II.	III.	IV.	V.	VI.	Mean.	Theory.
Carbon, . .	31·93	32·24	32·52	32·36	...	...	32·26	32·19
Hydrogen, .	3·09	2·62	2·58	2·74	...	...	2·76	2·39
Nitrogen, . .	...	...	...	...	...	...	...	4·17
Chlorine, . .	...	...	...	...	...	...	...	31·74
Platinum, .	29·44	29·30	29·60	29·30	29·40	29·26	29·38	29·51
								100·00

It will be seen that there is a slight excess both in the carbon and hydrogen of these analyses. This arises from the presence of a small quantity of lepidine, the platinum salt of the two bases being too nearly of the same degree of solubility to allow of separation by fractional crystallization. This source of error is much lessened in the other salts, their formation, in most cases, being a process of purification. Platinochloride of chinoline is very sparingly soluble in cold water, requiring 893 parts for solution at 60° F.

It is to be remembered, that all the chinoline compounds mentioned in this paper were made from a base procured by distillation of cinchonine with potash, the coal-chinoline requiring a tedious series of purifications, in addition to the fractional distillations, before it could be obtained pure enough for conversion into compounds fit for analysis. The platinum-salt is, however, more easily obtained in a pure state from the coal bases, than most other compounds of this alkaloid.

In the following table, the mean result of my analyses of the platinum-salt of chinoline is compared with those obtained by other observers,* whose numbers have been recalculated according to the present atomic weight of carbon.

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* GERHARDT, loc. cit.

	Hofmann.		**Gerhardt.**			**Bromeis.**			**Grev. Williams.**	
									Mean.	Calcul.
Carbon, .	32·06	...	32·99	32·46	32·51	33·31	33·42	33.33	32·26	32·19
Hydrogen,	2·58	...	3·14	3·14	3·28	2·71	2·83	2·68	2·76	2·39
Nitrogen,	...	...	4·42	...	...	3·98	4·21	4·00	...	4·17
Chlorine,	30·96	...	...	...	...	...	...	...	...	31·74
Platinum,	29·27	29·11	27·80	28·08	27·69	28·23	28·34	28·81	29·38	29·51
										100·00

Only the first of these analyses was made from a base extracted from coal-tar; all the others were obtained from chinoline, produced by destructive distillation of cinchonine with potash.

Laurent, by mixing hot alcoholic solutions of hydrochlorate of chinoline and bichloride of platinum, obtained, after twenty-four hours, fine yellow needles; but, on examination under the lens, it was found not to be a homogeneous crystallization, for a small quantity of little grains had also deposited.* I have not found that any observer, except myself, has subjected the bases produced from cinchonine to a systematic fractionation, before forming the platinum salt. The fraction analysed by me had been rectified fourteen times, and was nearly constant between 460° and 470°.

Aurochloride of Chinoline.—The only account of this beautiful salt I have been able to find is in Dr **Hofmann**'s paper, on the bases of coal-tar, where he merely states, that it corresponds in colour and other properties with the gold salt of aniline, but the latter appears† to be a yellow precipitate which rapidly becomes brown in the air, and, therefore, differs considerably from the chinoline salt, which is quite permanent under the same circumstances. As obtained by me from a specimen of chinoline of considerable purity, it was in the form of slender canary-yellow needles, sparingly soluble in cold water, and precipitating instantly on the addition of a solution of terchloride of gold, to a moderately strong solution of hydrochlorate of chinoline.

$$\left\{\begin{array}{l}\text{3·883 grains of aurochloride of chinoline dried at 212° gave, on ignition,}\\ \text{1·625 ... of gold.}\end{array}\right.$$

or, per cent.—

Experiment.	Theory. $\underbrace{(C_{18}\,H_7\,N, HCl + Au\,Cl_3)}$
41·85	42·00

Palladiochloride of Chinoline.—Dr **Hofmann** describes this salt in his paper, previously quoted, as resembling that from aniline, but M. **Müller**‡ states the latter to be *yellow*; I found, however, that when moderately concentrated solutions of chloride of palladium and hydrochlorate of chinoline are mixed, a copious deposit of *chestnut-brown* crystals takes place. This salt is moderately soluble in water. It requires a very strong heat to give pure metallic palladium.

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* **Gerhardt**, loc. cit., and **Laurent**, Ann. de Chim. et de Phys. [3] xxx., 368.

† **Gerhardt**, Traité, tome troisième, p. 86.

‡ Ann. der Ch. u. Pharm. lxxxvi., 368.

$\begin{cases} \text{3·423 grains of palladiochloride of chinoline gave, on powerful ignition in a porcelain capsule,} \\ \text{·725 ... palladium.} \end{cases}$

or, per cent.—

Experiment.	Theory.
	$\underbrace{(C_{18}\,H_7\,N,\,HCl, + Pd\,Cl}$
21·18	20·96

Cadmium Salt Chinoline.—The experiments of CROFT, and more especially VON HAUER, have shown that cadmium forms well-defined crystalline salts, with the chlorides of the alkalies, alkaline earths, and the chloride of ammonium. Before I became acquainted with the results of the latter chemist, I had been engaged in a series of experiments made with a view of extending our knowledge of the double salts formed by the hydrochlorates of the alkaloids with metallic chlorides. The information at present in our possession on this subject is very limited. The only salts of the class alluded to which have been analysed, are those formed with platinum, gold, palladium, and mercury. Now the salts of the latter metal vary greatly in constitution, and are, moreover, somewhat troublesome to analyse. I have, therefore, made a few experiments with a view to ascertain what other metals than those mentioned above, yield chlorides capable of combining with the alkaloids, to form well crystallized double salts. In the present communication, however, I only notice those formed by the chlorides of cadmium and uranyl with chinoline.

When moderately concentrated solutions of hydrochlorate of chinoline and chloride of cadmium are mixed, the fluid solidifies with rise of temperature to a snow-white mass of crystals. If the solutions are not too strong, they are obtained in the form of needles occupying a great bulk when in the mother liquor, but shrinking very much when pressed. They are less soluble in alcohol; I therefore used that fluid to wash them. The alcoholic washings, when kept for some time, deposit needles, often an inch long, but so silky and fragile as to be preserved of their original size with difficulty. They retain their colour perfectly, and, with the exception of losing two equivalents of water of crystallization, are quite unaltered by drying for a few hours at 212°. The salt volatilizes at a considerably higher temperature, without residue. The quantity at my disposal was very limited, and being, therefore, obliged to work on small quantities, I found it inconvenient to estimate the cadmium as oxide by the usual process, as the carbonate of cadmium, when precipitated from solutions containing chinoline at the boiling heat, not only has a strong tendency to pass through the filter, but adheres to the paper so strongly as to cause a loss of metal by reduction and volatilization during incineration. The precipitate was too light to be collected by decantation.

I obtained, however, an accurate result by precipitating with sulphuretted hydrogen, and collecting the sulphide of cadmium on a weighed filter.

I.	7·911	grains	of cadmium salt, dried at 212°, gave
	8·976	...	carbonic acid, and
	1·765	...	water.
II.	8·875	...	cadmium salt, dried at 212°, gave
	10·053	...	carbonic acid, and
	2·060	...	water.
III.	10·534	...	cadmium salt, dried at 212°, treated by Peligot's process, gave
	·42295	...	nitrogen.
IV.	6·997	...	cadmium salt chinoline, dried at 212°, gave
	2·885	...	sulphide cadmium.

	I.	II.	III.	IV.	Mean.	Calculation.		
Carbon, .	30·94	30·89	...	...	30·92	30·99	C_{18}	108
Hydrogen, .	2·48	2·58	...	...	2·53	2·29	H_8	8
Nitrogen, .	...	...	4·02	...	4·02	4·02	N	14
Chlorine, .	...	...	...	...	...	30·56	Cl_3	106·5
Cadmium, .	...	...	...	32·07	32·07	32·14	Cd_2	112
						100·00		348·5

It is evident, therefore, that the formula for the salt dried at 212° is—

$$C_{18} H_7 N, H Cl + 2 Cd Cl.$$

Several examples of salts of the same constitution occur in inorganic chemistry, some of which have been examined by M. Von Hauer, who terms them chlorobicadmiates. The chinoline salt, if merely dried by exposure to the air, contains two equivalents of water, for at 212° it loses 5·41 per cent.; theory requires 4·91. The excess arises from a little moisture adhering somewhat tenaciously to the crystals.

Hydrochlorate of Chinoline and Chloride of Uranyl.—If double carbonate of uranium and ammonia, dissolved in hydrochloric acid, is added to a strong solution of hydrochlorate of chinoline, the fluid rapidly becomes filled with short, brilliant, yellow needles, and in a few minutes the whole fluid solidifies, so that the vessel may be inverted without the contents escaping. From more dilute solutions, prismatic crystals are deposited, sometimes of considerable size. The salt is of a rich yellow colour, and is very soluble in water. It was quite free from any trace of ammonia. The mother liquid was removed by washing with alcohol. The quantity at my disposal was too small to allow of a complete examination of all its properties.

I.	6·760	grains	of uranium salt of chinoline, dried at 212°, gave
	7·900	...	carbonic acid, and
	1·686	...	water.
II.	5·135	...	uranium salt, dried at 212°, gave
	4·352	...	chloride of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon, ...	31·87	...	32·05	C_{18}	108
Hydrogen, ...	2·77	...	2·37	H_8	8
Nitrogen, ...	...	...	4·15	N	14
Chlorine, ...	...	20·97	21·07	Cl_2	71
Uranium, ...	...	...	35·61	Ur_2	120
Oxygen, ...	...	...	4·75	O_2	16
			100·00		337

The formula—

$$C_{18} H_7 N, H Cl, + (Ur_2 O_2) Cl,$$

appears, therefore, to be the correct expression of the analysis, and it agrees in constitution with the anhydrous ammoniochloride of uranyl of Peligot. It is my intention to examine the double compounds of uranium with other organic bases.

Binoxalate of Chinoline.—The great discrepancy in the results of Runge and Hofmann with regard to the oxalate of chinoline, made me desirous of ascertaining the nature of this salt. According to the former chemist, leukol (chinoline) has such a great tendency to form a crystalline oxalate, that this property is its marked characteristic; Hofmann, on the other hand, could only obtain it in the form of a confused, radiated, glutinous mass, deposited when the solution had reached a certain state of concentration. I found, however, that if 24·3 parts of chinoline are added to 16·5 parts dry oxalic acid, dissolved in a small quantity of water, the whole solidifies to a white crystalline mass, of the consistence of soft cheese. The salt cannot be obtained pure by a random admixture of the ingredients, as, although the chief tendency appears to be to form the binoxalate, yet other compounds are also formed in sufficient quantity to prevent constant analytical results from being obtained, unless the above proportions are used. The salt, before being employed for analysis, must be recrystallized from alcohol once or twice, when it forms fine silky needles. It is partially decomposed by exposure for two days to 212°, with evolution of chinoline, a salt being formed intermediate in composition between the binoxalate and quadroxalate. It is necessary, therefore, to dry it for analysis *in vacuo* over sulphuric acid.

6·995 grains of binoxalate of chinoline, dried *in vacuo*, gave
15,407 ... carbonic acid, and
2,798 ... water.

	Experiment.	Calculation.		
Carbon, ...	60·07	60·27	C_{22}	132
Hydrogen, ...	4·44	4·11	H_9	9
Nitrogen, ...	...	6·39	N	14
Oxygen, ...	...	29·23	O_8	64
		100·00		219

Nitrate of Chinoline.—In some respects my experiments on this salt tally with those of Dr Hofmann, in others they differ considerably. It was obtained by the last-named chemist by allowing a mixture of leukol and dilute nitric acid to rest under a bell-jar; after some time the salt crystallized in confused concentric needles, which were obtained white and dry by pressure between folds of filtering paper. He does not appear to have analysed it. I found that if, after an excess of nitric acid slightly diluted was added to chinoline, the fluid was evaporated on the water-bath, a pasty mass was obtained, which solidified on cooling. From a hot alcoholic solution fine white needles soon deposited, which were infusible at 212°, and unalterable in the air. Dr Hofmann, on the contrary, found his salt to fuse on moderate heating, and to rapidly become blood-red by exposure to the air; these are, evidently, the characters of an impure substance. The nitrate of chinoline was burnt with oxide of copper, a long column of copper turnings being placed in the front of the tube.

6·590 grains of nitrate of chinoline, dried at 212°, gave
13·516 ... carbonic acid, and
2·530 ... water.

	Experiment.	Calculation.		
Carbon,	55·94	56·25	C_{18}	108
Hydrogen,	4·27	4·17	H_8	8
Nitrogen,	...	14·58	N_2	28
Oxygen,	...	25·00	O_6	48
		100·00		192

Chinoline gives a very marked reaction with strong fuming nitric acid, and which also shows its great stability. If a few drops of the base are allowed to trickle down the side of a test-tube, and a small excess of the acid is added, the two combine with violence, the portion of alkaloid adhering to the sides is converted by the fumes of the acid into long needles, and, when cold, the whole fluid solidifies to a beautifully white crystalline mass of pure nitrate. No nitrochinoline, or any other decomposition product, is formed, if the base be free from impurities.

Bichromate of Chinoline.—Gerhardt, in describing this salt,* merely states, that chromic acid in solution gives, with pure chinoline, an orange-yellow crystalline precipitate, and that the dry acid decomposes the base with inflammation, Dr Hofmann, in his paper on the coal bases, previously referred to, states that, a short time after he had commenced the investigation of leukol, he was inclined to consider it the same as that Gerhardt obtained by the action of hydrate of potash on quinine, cinchonine, and strychnine. He says, however, that he soon convinced himself that they were totally distinct, their behaviour towards a solution of chromic acid being quite dissimilar, for while chinoline and its salts gave a beautiful orange-yellow crystalline precipitate, leukol was oxidized and converted into a black resinous oil. Subsequently,* LIEBIG announced, upon the authority of experiments made by HOFMANN, that perfectly pure leukol gave the same crystalline precipitate with chromic acid. I have not been so successful as M. HOFMANN; for, although the chinoline and lepidine procured by destructive distillation from cinchonine have, in my hands, given salts of extreme beauty and purity with chromic acid, I have failed to obtain the same result with either the chinoline or lepidine from coal-tar. I have also boiled the bases from the latter source with dilute chromic acid, to destroy impurities, and then separated them by distillation with potash, but they merely gave an oily precipitate with chromic acid. When dissolved in hydrochloric acid, and bichromate of potash is added, the same result occurs. I even took a platinum salt of coal-lepidine, which yielded, on combustion, the numbers detailed in Analysis I., p. 398; and, after having reobtained the base by distillation with potash, endeavoured to procure from it a crystalline chromate, but in vain, a red oil being the only product. It is true, that when I added dilute chromic acid to chinoline from coal-tar, the sides of the tube acquired a coating of very minute brilliant points, which reflected light with a peculiar satin-like lustre; but the lens resolved them into oily globules. The following experiments were, therefore, made upon chinoline from cinchonine. Neither GERHARDT nor HOFMANN have analysed the salt.

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* Traité de Chimie Organique, troisième partie, p. 150.

The beauty of the bichromate of lepidine described in my last paper,† induced me to ascertain the composition and properties of the homologue next below it, in the anticipation that its outward appearance would be equally striking. But there are some slight differences in the two bodies; bichromate of chinoline is still less soluble than the other salt, and this prevents the crystals from being readily procured of so large a size. When dry, it is much more violently decomposed by heat than the lepidine compound. The fixed product is, however, the same, namely, green oxide of chromium and carbonaceous matter. If the dry salt be placed in a capsule, and heat be very gradually applied, no change at first takes place, but suddenly it takes fire with explosive violence, and the greater part of the green oxide and carbon is projected. I prepared the salt for analysis by adding dilute chromic acid in excess to pure chinoline; at first the product is somewhat resinous, but immediately it is touched with a glass rod, it becomes gritty and crystalline. The solid is then filtered off, the mass slightly washed, dissolved in boiling water, filtered to remove traces of an oily impurity, and, on cooling, the fluid becomes filled with brilliant yellow needles arranged in groups. It may be dried at 212° with safety, provided adhering moisture has been removed as much as possible by pressure between folds of filtering paper. If the salt is previously moistened with hydrochloric acid, it may be ignited without explosion, and the green oxide estimated with accuracy. The combustion was made with oxide of copper.

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* Chem. Gaz., vol. iii., p. 251 (1845). Proc. of Chem. Soc., April 7, 1845.

† Trans. Roy. Soc. Edin., vol. ~~xxi~~. part ii.

I.	6·827	grains of	bichromate of chinoline, dried at 212° gave
	11·284	...	carbonic acid, and
	2·146	...	water.
II.	6·381	...	bichromate of chinoline, dried at 212°, gave, on ignition,
	2·072	...	green oxide of chromium.
III.	5·534	...	bichromate of chinoline, dried at 212°, gave, on ignition,
	1·787	...	green oxide of chromium.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, . .	45·08	...	...	45·11	C_{18}	108
Hydrogen, . .	3·49	...	...	3·34	H_8	8
Nitrogen, . .	...	...	...	5·85	N	14
Chromium, . .	...	22·40	22·28	22·31	Cr_2	53·4
Oxygen, . .	...	...	...	23·39	O_7	56·0
				100·00		239·4

Density of the Vapour of Chinoline.—In Dr Hofmann's paper on the coal bases, he states that a determination of the density of the vapour of leukol (chinoline) failed, owing to its leaving a yellow residue on distillation. I have not found this circumstance to operate sufficiently in the case of chinoline from cinchonine, to cause more error than is usually found in determining the vapour densities of bodies obtained by fractional distillation, and having so high a boiling point. The specimen used, boiled in the fourteenth rectification between 460° and 470° F.

Temperature of air, . .	13° centigrade.
... vapour, . .	277° ...
Pressure, ...	751 millimetres.
Capacity of balloon, . .	330 cent. cub.
Residual air, . . .	17·5 ...
Excess of weight of balloon, .	0·4980 grammes.

The formula

$$C_{18}\,H_7\,N$$

requires

18 volumes carbon vapour, . .	0·829	. 18 =	14·922
14 ... hydrogen, . . .	0·0692	. 14 =	·9688
2 ... nitrogen, . . .	0·9713	. 2 =	1·9426
			17·8334

$$\frac{17{\cdot}8334}{4} = 4{\cdot}4583$$

Experiment.	Theory. $C_{18}\,H_7\,N = 4$ volumes.
4·5190	4·4583

Action of Iodide of Methyl on Chinoline.

Hydriodate of Methyl-Chinoline.—When an excess of iodide of methyl is added to chinoline, and the mixture, inclosed in a pressure tube, is heated for ten minutes to 212°, combination is perfectly effected, a finely crystallized hydriodate resulting. If this salt, which is perhaps more correctly called iodide of methyl-chinoline-ammonium, is treated in the cold with excess of oxide of silver, a strongly alkaline solution is obtained, containing the hydrated oxide of the ammonium base. The solution possesses little stability; on heating with potash, an excessively pungent odour is evolved, acting strongly on the eyes and mucous membrane of the nose. The solution reddens turmeric paper as powerfully as solution of caustic potash, and instantly restores the colour of reddened litmus. The reactions of this base, generally, are the same as those of the ethyl compound next to be described. The smell of a volatile base, a product of the decomposition of methyl-chinoline, is evolved from the moment of its formation; it appears to be methylamine.

By alternate precipitation of the hydriodate by nitrate of silver, hydrochloric acid, and bichloride of platinum, after removal of the chloride of silver, a sparingly soluble platinum salt was obtained. The following is the result of its analysis:—

I.	8·930	grains of	platinochloride of methyl-chinoline, gave
	11·350	...	carbonic acid, and
	2·496	...	water.
II.	8·515	...	platinochloride of methyl-chinoline gave
	10·777	...	carbonic acid, and
	2·303	...	water.
III.	7·861	...	platinochloride of methyl-chinoline gave
	2·217	...	platinum.
IV.	5·165	...	platinochloride of methyl-chinoline gave
	1·456	...	platinum.

	Experiment.				Mean.	Calculation.		
	I.	II.	III.	IV.				
Carbon,	34·66	34·52	...	...	34·59	34·33	C_{20}	120
Hydrogen,	3·11	3·00	...	...	3·06	2·86	H_{10}	10
Nitrogen,	...	...	...	...	...	4·01	N	14
Chlorine,	...	...	...	...	...	30·47	Cl_3	106·5
Platinum,	...	...	28·20	28·19	28·20	28·33	Pt	99
						100·00		349·5

Methyl-chinoline is, therefore, isomeric with lepidine, but has no other point of resemblance. The decompositions of the hydriodate almost exactly resemble those of the ethyl base, and, as the atomic weight of the latter, being higher, gave it an advantage for experiment, I selected it for the purpose.

Action of Iodide of Ethyl on Chinoline.

Hydriodate of Ethyl-Chinoline.—No action takes place on the mere addition of excess of iodide of ethyl to chinoline, but if the tube containing the mixture be sealed and exposed for some hours to a temperature of 212°, the whole becomes a mass of crystals. When this is the case, and the tube cools, the end may be cut off, a tube bent twice at right angles attached by means of a cork, and, the pressure-tube being immersed in the water-bath, the excess of iodide of ethyl distilled over. The crystals are then dissolved out in a small quantity of hot alcohol, and the solution allowed to cool. The first crop is of a rich yellow colour, becoming of a pale lemon tint on recrystallization. In the state in which the salt is thus obtained it possesses the property of becoming a deep blood-red at 212°, and regaining its normal tint on cooling; this peculiarity becomes much lessened by a repetition of the process. The crystals appear to be cubic, and are easily obtained of considerable size. They dissolve more readily in water than alcohol, but the latter is the best solvent for the purposes of crystallization.

I. 6·336 grains of hydriodate of ethyl-chinoline, dried at 212°, gave
10·810 ... carbonic acid, and
2·513 ... water.

II. 6·684 ... hydriodate of ethyl-chinoline, dried at 212°, gave
5·457 ... iodide of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	46·53	...	46·32	C_{22}	132
Hydrogen, . . .	4·41	...	4·21	H_{12}	12
Nitrogen, . . .	...	...	4·91	N	14
Iodine . . .	...	44·12	44·56	I	127
			100·00		285

Platinum Salt of Ethyl-Chinoline.—After adding nitrate of silver to a solution of hydriodate of ethyl-chinoline, as in making the iodine determination last mentioned, the excess of silver was removed by the addition of hydrochloric acid, the liquid filtered, and evaporated to a moderate bulk, on the addition of bichloride of platinum, a rich golden-yellow precipitate of sparing solubility was obtained; it was first washed with a little water, and afterwards with alcohol.

6·813 grains of platinochloride of ethyl-chinoline gave
1·847 ... platinum.

Agreeing with the formula

$$C_{22}\,H_{11}\,N,\,H\,Cl + Pt\,Cl_2.$$

	Experiment.	Calculation.		
Carbon, ...	...	36·31	C_{22}	132
Hydrogen, . . .	...	3·30	H_{12}	12
Nitrogen, ...	...	3·85	N	14
Chlorine, ...	...	29·30	Cl_3	106·5
Platinum, ...	27·11	27·24	Pt	99
		100·00		363·5

It is evident that ethyl-chinoline is isomeric with cryptidine, the new base to be described further on.

Action of Oxide of Silver on Iodide of Ethyl-Chinoline.

Hydrated Oxide of Ethyl-Chinoline-Ammonium.—A solution of iodide of ethylchinoline is decomposed with ease by oxide of silver, even in the cold, a colourless strongly alkaline fluid being formed, containing the fixed base corresponding to the hydrated oxide of tetrethylammonium. The solution instantly reddens turmeric paper, and restores the colour of reddened litmus. It precipitates solutions of sulphate of copper, sesquichloride of iron, acetate of lead, and corrosive sublimate. The addition of a small quantity to a red solution of bichromate of potash renders it yellow, by neutralizing the second equivalent of chromic acid. The solution of the base decomposes chloride of ammonium, liberating the ammonia freely.

The heat of a water-bath decomposes the solution of the hydrated oxide, with production of a splendid crimson colour, the sides of the basin where the liquid has dried becoming a brilliant emerald green, passing in a few seconds to a blue of great beauty and intensity. These colours, like those to be mentioned presently, evidently depend upon oxidation, and would require a very large amount of material to follow out in detail. When the solution of hydriodate of ethylchinoline is heated on the water-bath with excess of oxide of silver, a volatile product is evolved, acting strongly upon the eyes.

Action of Sulphate of Silver upon Hydriodate of Ethyl-Chinoline.

If hot solutions of sulphate of silver and hydriodate of ethyl-chinoline are mixed, double decomposition ensues, without any further action taking place, the solution of sulphate of ethyl-chinoline remaining colourless, and the iodide of silver separated being of the normal tint; but, if it be attempted to concentrate the solution by evaporation on the water bath, it undergoes a curious metamorphosis, the sides of the dish, where the solution has dried, become a deep pure blue, but, as the evaporation proceeds, the solution becomes crimson, and when dry, the mass is so deep in tint, as to be nearly black. The dry substance has a slight coppery lustre, like that which indigo possesses when rubbed. It dissolves in water, the solution being of the most gorgeous crimson, becoming rose-coloured by addition of ammonia, while hydrochloric or nitric acids convert it to a scarlet. The colour is tolerably stable, requiring a considerable excess of bromine water to decompose it, the fluid then becoming reddish-brown.

The crimson liquid undoubtedly contains the sulphate of a new base, apparently a product of oxidation of ethyl-chinoline. The reactions upon which this supposition is founded, are the following:—If solution of potash be added to the crimson solution, the colouring matter is almost entirely precipitated, and, if the experiment be successful, the solution merely retains a slight blue tinge. This precipitate appears to be the new base, although probably in a very impure state. When first thrown down, it has a beautiful reddish-violet colour, like that of the crystallized sesquichloride of chromium. It may be washed with water on a filter, being sparingly soluble; it dissolves readily in alcohol, forming a fine crimson fluid, which precipitates a spirituous solution of chloride of mercury. The base dissolves readily in hydrochloric acid, the solution giving a voluminous precipitate with bichloride of platinum. The platinum salt, after well washing with water, was burnt for the percentage of metal, to ascertain whether its atomic weight was higher or lower than that of ethyl-chinoline.

$$\left\{\begin{array}{lll} 2{\cdot}081 & \text{grains of platinum salt gave} \\ {\cdot}492 & \ldots \quad \text{platinum.} \end{array}\right.$$

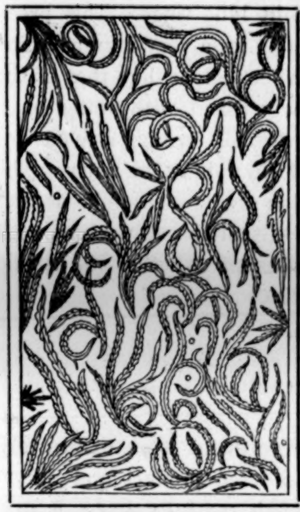
212·2	157·0
Atomic weight of new base.	Atomic weight of ethyl-chinoline.

The atomic weight which is derived from this experiment is so excessively high, that no simple relation is apparent between the red product and ethyl-chinoline. The single platinum determination, although carefully made, is evidently insufficient to enable any speculation to be made as to the nature of the decomposition.

It is known that most nitryl bases (those composed solely of the alcohol radicals being the chief exceptions) yield colours by the action of oxide of silver on the ammonium compounds formed with methyl, ethyl, and amyl, but I think none yet worked on yield such magnificent tints as those mentioned in this paper. The subject has another and much greater point of interest than the mere formation of coloured reactions, however beautiful they may be, inasmuch as the careful following out of the decompositions on the large scale promises to assist us in acquiring a knowledge of the constitution of alkaloids of this class. I have therefore promised myself to examine the matter more fully, when the other investigations with which I am now occupied are concluded.

Action of Iodide of Amyl on Chinoline.

Hydriodate of Amyl-Chinoline.—Iodide of amyl reacts with comparative slowness upon chinoline. It is necessary to keep the materials in a pressure-tube for some hours at 212° to effect combination. The iodide crystallizes from alcohol with extreme readiness, and when evaporated slowly upon flat surfaces presents under the lens very peculiar and beautiful forms. On one occasion, after dissolving the iodide in alcohol in a beaker, the fluid was poured into another vessel, and the solution remaining on the sides crystallized in the manner I have endeavoured to illustrate in the annexed sketch. The figure represents the forms about four times the natural size.



The hydriodate of amyl chinoline gave, in a determination of the percentage of iodine, the following numbers.

9·882 grains hydriodate amyl-chinoline gave
7·086 ... iodide silver.

	Experiment.	Calculation.		
Carbon,	...	51·38	C_{28}	168
Hydrogen,	...	5·50	H_{18}	18
Nitrogen,	...	4·28	N	14
Iodine,	38·75	38·84	I	127
		100·00		327

The fluid from which the iodine had been precipitated was treated with hydrochloric acid in excess, the chloride of silver removed by filtration, and the fluid evaporated to a moderate bulk, excess of bichloride of platinum was then added, and the precipitated platinum salt washed, first with a little water, and then with a mixture of alcohol and ether. The platinochloride of amyl-chinoline is only sparingly soluble in water, it was dried at 212°, and burnt with chromate of lead and copper turnings.

6·756 grains of platino-chloride of amyl-chinoline, gave
10·233 ... carbonic acid, and
2·813 ... water
7·150 ... platinochloride of amyl-chinoline gave
1·733 ... platinum.

	Experiment.	Calculation.		
Carbon,	41·31	41·43	C_{28}	168
Hydrogen,	4·63	4·44	H_{18}	18
Nitrogen,	...	3·45	N	14
Chlorine,	...	26·26	Cl_3	106·5
Platinum,	24·24	24·42	Pt	99
		100·00		405·5

Action of Chlorine on Chinoline.—According to GERHARDT,* chlorine converts chinoline into a black resin, but my experiments show that it acts in a very different manner, if care be taken to prevent rise of temperature. On dropping chinoline into a large glass vessel of the gas, and leaving it for twelve or fourteen hours, a yellow oil is produced, which, on treatment with water, leaves a white insoluble matter, which I have not yet had an opportunity of studying more in detail.

Action of Chloride of Acetyl on Chinoline.—Chloride of acetyl on being added to chinoline, develops much heat, and on evaporation at 212°, a crystalline mass was obtained, but so deliquescent, as to be unfit for examination.

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* Traité, troisième partie, p. 150.

On the Chinoline Series as it occurs in Coal-Tar.

In my paper "On some of the Basic Constituents of Coal-Naphtha and on Chrysène," I ventured to express a belief, that chinoline was not the only member of the group to which it belongs present in coal-tar; and feeling assured that other homologues remained to be discovered, I was desirous of testing the accuracy of the supposition. Owing to the kindness of Mr GEORGE MILLER of Dalmarnock, I was enabled to obtain fifty gallons of coal-oil of a very high boiling point, and of a density greater than that of water. It was shaken with sulphuric acid to extract the alkaloids, and the acid fluid after dilution with water was treated with excess of lime, and distilled as long as any came over. As the amount of bases of the Dippel series present, was not large, the product being of such a high boiling point, I did not add potash to separate the more soluble portion, but only collected that part which, from its density and insolubility, sank to the bottom of the fluid accompanying it in the distillation. By means of a tap funnel, the basic-oil was separated from the chief part of the water accompanying it, which contained some of the pyridine series in solution. The bases thus obtained are exceedingly impure, and contain aniline and some non-basic substances. It being probable, that toluidine and even other members of the same series might be present, I thought that as apparently insurmountable difficulties prevented their separation from the chinoline series by means of oxalic acid, or similar methods, their presence might nevertheless be made manifest through their products of decomposition. With this view, I treated the mixed bases with nitrite of potash and hydrochloric acid, in the manner indicated by HUNT,[*] and by this means effectually decomposed all traces of the aniline group present. But the amount of oil heavier than water containing the hydrates of phenyl and cresyl was too small to allow of my further examining them. I propose, however, to return to the subject at a future time.

The acid fluid, after decantation from the heavy oil last alluded to, was then placed in a retort, and a jet of steam sent through the tubulature to the bottom of the liquid; by this means, many non-basic impurities were removed, and amongst them a white crystalline solid distilled over with the steam, and which eventually proved to be naphthaline. The acid fluid in the retort, after being filtered through pulverized charcoal, to separate resinous matters not volatilized, was treated with potash, to liberate the base, which was then separated by a tap funnel, and completely dried by digestion with sticks of potash. It is proper to mention, that by this treatment, the boiling point of the bases was considerably raised, the aniline being removed, which boils 100° below chinoline.

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[*] SILLIMAN's Journal 1849; Chem. Gaz., Jan. 1850; GERHARDT, Traité, tome 3^{me}, p. 83; HOFMANN, Quart. Jour. Chem. Soc.

The bases, as purified by the above method yield, on distillation, fractions from 350° to 525°, Considerably more than one hundred distillations were made before sufficient separation had taken place, to justify me in making any analyses.

Chinoline having already been proved to exist in coal-tar, I began the experiments by searching for lepidine. This base, which was discovered by me* among the volatile alkaloids procured by distilling cinchonine with potash, has the formula,

$$C_{20}\,H_{9}\,N,$$

which was established by analyses of the platinum salt, nitrate, hydrochlorate, and bichromate, confirmed also by a determination of its vapour density.

As obtained from coal-tar, lepidine is in the form of an oil, having an odour almost exactly the same as that from cinchonine. But it is impossible by distillation alone, even after the treatment of the crude base with nitrous acid, to procure it in the same state as from the source where it was first found. Coal-lepidine, therefore, yields salts which, for the most part, crystallize with incomparably greater difficulty than that from cinchonine. When dissolved in an acid, although the solution is perfect, there is always an after-odour evolved, somewhat like naphthaline, unless the base has been purified with great care. I was unable, by any means at at my disposal, to obtain the bichromate in a crystalline state, whereas the lepidine from cinchonine yields a beautiful salt, in brilliant yellow needles half-an-inch long. Even when coal-lepidine is boiled with diluted chromic acid for an hour, the base, when separated and redistilled, gives an oily precipitate with chromic acid; moreover, the same occurs if the base be dissolved in hydrochloric acid, and a solution of bichromate of potash is added, as has been previously mentioned in describing the bichromate of chinoline. The red oil obtained in this manner from coal-lepidine may be kept for weeks without showing any tendency either to crystallize or decompose; even the base obtained from a platinum salt of considerable purity, behaved in the same way. On the other hand, coal-lepidine reacts with nitric acid and some other re-agents, like that from cinchonine.

If it were not for the decisive manner in which the fact of Dr HOFMANN's having obtained the crystalline precipitate with leukol and chromic acid has been announced, I should have felt justified in asserting that the bases derived from the two sources were isomeric but not identical; but as the last-named chemist's well-known accuracy prevents me from entertaining a belief in the possibility of an error of experiment, I can only express my regret, that I am ignorant of the method of purification adopted by him. The most satisfactory mode of explanation of the differences in the properties of the coal and cinchonine series, as obtained by me, is that they are in a peculiar molecular condition, analogous in some respects to the phenomena known in the cases of quinine, the amylic alcohol, and many other bodies, instances of which are daily becoming more numerous. Chemists are aware that even variations in the density and boiling point of the same fluid, when in different states, have been observed; and I may mention, as corroborative of this, that with bases distilled the same number of times, lepidine as pure as I could procure it from both sources differed in boiling point by 25° F.; for the lowest fraction of coal-lepidine that gave correct results on analysis distilled between 485° and 495°, whereas the lowest fraction of the same base from cinchonine, boiled between 510° and 520° F. Another fact which seems corroborative of the supposition that Dr Hofmann obtained the chinoline from coal-tar in the state in which I procured it from cinchonine, is found in the circumstance that the density of chinoline from coal-tar was ascertained by him to be 1·081, or very near the same number as in my determination of the density of the same base from cinchonine, viz., 1·085. But the coal bases examined by me were lighter than this; for even the lepidine from the the last source had a density of only 1·072 at 60° F., being actually lighter than the homologue, one step below from cinchonine.

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* Trans. Royal Soc. Edin., vol. xxi., pt. 27.

I have observed with the pyridine series, as obtained from bone-oil, coal-naphtha, and bituminous shale, that considerable differences are found in their power of forming crystalline salts, and it is, therefore, most probable that the same distinctions exist between them that are met with in the case of the bases from coal and cinchonine. I trust eventually to be able to elucidate some of these points, by subjecting chinoline from both the above substances to the action of polarized light.

The lepidine platinum salt, from cinchonine, precipitates at once in a pulverulent state; but from coal it is for a few seconds soft and resinous, but soon becomes hard and crystalline. The following are my analyses of it from the latter source:—

I.	9·216	grains	crystallized platinochloride of lepidine from fraction boiling 485°-95° 14th rectification, dried at 212°, gave
	11·652	...	carbonic acid, and
	2·466	...	water.
II.	5·954	...	platinochloride lepidine from fraction 485°-95°, gave
	1·673	..	platinum.
III.	8·754	...	platinochloride lepidine from fraction 495°-505°, 14th rectification, gave
	11·101	...	carbonic acid, and
	2·452	...	water.
IV.	5·554	...	platinochloride lepidine, same as last, gave
	1·575	...	platinum.
V.	5·047	...	platinum salt, 485°-95° crystallized, gave
	1·417	...	platinum.

	Experiment.					Mean.	Calculation.		
	I.	II.	III.	IV.	V.				
Carbon,	34·48	...	34·58	...	...	34·53	34·33	C_{20}	120
Hydrogen,	2·97	...	3·11	...	...	3·04	2·86	H_{10}	10
Nitrogen,	...	...	...	...	...	...	4·01	N	14
Chlorine,	...	...	...	...	...	...	30·47	Cl_3	106·5
Platinum	...	28·09	...	28·36	28·08	28·17	28·33	Pt	99
							100·00		349·5

In the following table, the mean of these results is compared with my analyses of the platinum salt from the cinchonine bases.

	Coal-Tar. Mean.	Cinchonine. Mean.	Theory.
Carbon, . . .	34·53	34·04	34·33
Hydrogen, . . .	3·04	2·95	2·86
Nitrogen, . . .	...	...	4·01
Chlorine, . . .	...	...	30·47
Platinum, . . .	28·17	28·13	28·33

Action of Iodide of Ethyl on Lepidine.

Hydriodate of Ethyl-Lepidine.—As it was evident that the process for preparing this compound, and from it the platinum salt, was one of purification, I thought that I should, by this means, obtain nearer results on analysis than was the case with the experiments last quoted; and the following may be considered as confirming the truth of the supposition.

Coal-lepidine, sealed in a tube with excess of iodide of ethyl, and exposed for some hours to 212°, yields a mass of brown needles, which, on recrystallization from alcohol, are of a brilliant canary yellow. They have the same property of becoming red at 212° as the corresponding salt of chinoline, although scarcely to the same degree.

{7·111 grains iodide ethyl-lepidine gave
{5·595 ... iodide silver.

or per cent.—

	Experiment.	Calculation.		
Carbon, . .	...	48·16	C_{24}	144
Hydrogen, . .	...	4·68	H_{14}	14
Nitrogen, . .	...	4·68	N	14
Iodine, . .	42·52	42·48	I	127
		100·00		299

Platinum Salt of Ethyl-Lepidine.—This salt was obtained in the same manner as the corresponding one of ethyl-chinoline. It is, at the first moment of precipitation, somewhat soft, but soon becomes hard and crystallized. It was pulverized and well washed with a mixture of alcohol and ether previous to analysis.

I. 8·374 grains platinochloride ethyl-lepidine gave
11·667 ... carbonic acid, and
2·880 ... water.

II. 6·377 ... platinochloride ethyl-lepidine gave
1·688 ... platinum.

III. 4·952 ... platinochloride ethyl-lepidine gave
1·317 ... platinum.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, . .	38·00	...	...	38·14	C_{24}	144
Hydrogen, . .	3·82	...	...	3.71	H_{14}	14
Nitrogen, . .	...	...	...	3·71	N	14
Chlorine, . .	...	...	...	28·21	Cl_3	106·5
Platinum, . .	...	26·47	26·60	26·23	Pt	99
				100·00		377·5

Density of Vapour of Lepidine.—In my former paper on the chinoline bases, I gave 5·14 as the density of the vapour of lepidine, as found by experiment, and I was desirous of ascertaining that of the same base, as extracted from coal-tar, in order to serve as a comparison. It is remarkable to observe the difference which an increment of C_2H_2 has in modifying the power of substances to resist the decomposing influence of heat. While, in taking the density of chinoline at 531° F., being 71° above its boiling point, the fluid condensed in the balloon was almost colourless, lepidine after exposure under the same circumstances to only 523° F., or 28° above its boiling point, had become nearly black; this darkening, caused by separation of carbon, prevented me from making the experiment at a temperature as much above the boiling point as in the case of chinoline, and the two sources of error have the effect of making the density come out somewhat too high.

Temperature of air, . .	15° centigrade.
... vapour, .	273° ...
Pressure, ...	755 millimetres.
Capacity of balloon . .	331 cent. cub.
Excess of weight of balloon, .	·6745 grammes.
Residual air, . . .	6 cent. cub.
Density, . . .	5·15

The formula

$$C_{20}H_9N$$

requires the following numbers:—

20 volumes carbon vapour, . . . =0·8290 . 20=16·580
18 ... hydrogen, ... =0·0692 . 18= 1·2456
2 ... nitrogen, ... =0·9713 . 2= 1·9426

$$\frac{19{\cdot}7682}{4}=4{\cdot}94205$$

Density of vapour of lepidine from cinchonine.	Density of vapour of lepidine from coal.	Theory. $C_{20}H_9N=4$ volumes.
5·14	5·15	4·94

I. 7·921 grains platinochloride of cryptidine, after treatment with nitric acid, &c., dried at 212°, gave
10·603 ... carbonic acid, and
2·453 ... water.

II. 5·108 ... platinochloride of cryptidine, same as last, gave
1·392 ... platinum.

	I. & II.	Calculation.
Carbon, . .	36·51	36·31
Hydrogen, . .	3·44	3·30
Nitrogen, . .	...	3·85
Chlorine, . .	...	29·30
Platinum, . .	27·25	27·24
		100·00

A very perceptible increase in the carbon is, therefore, obtained by the
val of the impurity rendered insoluble by means of nitric acid.

Chinoline is, therefore, the first of a series of homologous nitryl ba
which three members are now known, viz.:—

Chinoline, . .	$C_{18} H_7 N$
Lepidine, . .	$C_{20} H_9 N$
Cryptidine, . .	$C_{22} H_{11} N$

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XXVII. *On Fermat's Theorem.* By H. F. Talbot, Esq., F.R.S., &c.

(Read 7th April 1856.)

It is well known that no satisfactory demonstration has ever been given of Fermat's celebrated theorem, which asserts that the equation $a^n = b^n + c^n$ is impossible, if *a, b, c*, are whole numbers, and *n* is any whole number greater than 2. In Legendre's *Théorie des Nombres*, he demonstrates the cases of $n=3$, $n=4$, and $n=5$, the latter only in his Second Supplement. In Crelle's *Mathematical Journal*, ix. 390, M. Dirichlet, a mathematician of Berlin, has demonstrated the case of $n=14$, but I am not aware whether his demonstration is considered successful. Legendre informs us (Second Supplement, p. 3) that the Academy of Sciences, with the view of doing honour to the memory of Fermat, proposed, as the subject of one of its mathematical prizes, the demonstration of this theorem; but the Concourse, though prolonged beyond the usual term, produced no result.

It is a remarkable circumstance, however, that Fermat himself was in possession of the demonstration, or at least believed himself to be so, and he describes his demonstration as being a wonderful one—*mirabilem sanè*.* He does not say that the theorem itself is wonderful, but his demonstration of it; from which I think it likely that he meant to say that it was very remarkable for its shortness and simplicity.

Since, however, subsequent mathematicians have failed to discover any demonstration, much less an extremely simple one, of this celebrated theorem, it has been surmised that Fermat deceived himself in this matter, and that his demonstration, if it had been preserved to us, would have proved unsatisfactory. Legendre says, "Fermat a pu se méprendre sur l'exactitude ou la généralite de sa démonstration."

Nevertheless, in considering this question attentively, I have found that there is one case in which Fermat's theorem admits of a singularly simple demonstration; and as I do not find it noticed in any mathematical work to which I have been able to refer, I think it worthy of being brought under the notice of mathematicians. It may possibly prove to be a step in the right direction towards the recovery of Fermat's lost demonstration. It is, moreover, in itself a very extended and remarkable theorem, although less so than that of Fermat.

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* "Cubum autem in duos cubos aut quadrato-quadratum in duos quadrato-quadratos et generaliter nullam in infinitum, ultra quadratum, potestatem in duos ejusdem nominis fas est dividere. Cujus rei demonstrationem mirabilem sanè detexi. Hanc marginis exiguitas non caperet."—Fermat, *Notes sur Diophante*, p. 61.

The case which admits of this simple demonstration, is that in which one of the three numbers a, b, c, is a prime number; and it divides itself into the two following theorems:—

Let a be any prime number, then,

Theorem I. If n is any odd number greater than 1, the equation $a^n = b^n + c^n$ is impossible.

Theorem II. If n is any number, odd or even, greater than 1, the equation $a^n = b^n - c^n$ is impossible.

But Theorem II. admits a case of exception, viz., that whenever $b - c = 1$, the theorem remains undemonstrated. When $n = 2$, this case of exception actually occurs, because $a^2 = b^2 - c^2$ is possible, although a be a prime number. For example, when $a = 3$, $3^2 = 5^2 - 4^2$.

Such a case of exception, however, does not occur when $n = 3$, or $n = 4$, or, $n = 5$, as LEGENDRE has demonstrated. But that is no reason why it should not occur with other values of n. And therefore it appears that the generality of FERMAT'S theorem is assailable in this direction; a fact which deserves the attention of mathematicians, especially as FERMAT himself does not appear to have adverted to it.

In order to demonstrate these propositions, I will recall to mind some of the leading principles of the Theory of Numbers.

1. If a prime number does not divide either of the whole numbers A or B, it does not divide their product AB. LEGENDRE, p. 3, gives a very rigorous demonstration of this important theorem.

2. When a number has been divided into its prime factors, it cannot be divided into other prime factors different from the first ones.

3. The product of any number of primes, cannot be equal to the product of any number of other primes different from the first ones.

And here it may be observed, that although these products cannot be equal, nothing prevents them from approximating as closely as possible to equality, *i. e.*, differing by a single unit. For example, the product of the

three primes $7 \times 17 \times 83 = 9877$

that of $2 \times 11 \times 449 = 9878$

that of $3 \times 37 \times 89 = 9879$.

These things being premised, we may proceed as follows:—

Demonstration of Theorem I.

Let us suppose, if possible, $a^n = b^n + c^n$. Then because n is an odd number, $b^n + c^n$ is divisible by $b + c$. Let the quotient be Q. Therefore $a^n = \overline{b+c} \cdot Q$. Now since a is prime, the first side of the equation is the product of the n factors $a \times a \times a \times$ &c. Consequently the second side of the equation is the product of the same n factors. And it cannot possibly have any other. Therefore, since it has the factor $b+c$, this factor must itself be either $= a$, or divisible by a.

But we shall now proceed to show that $b+c$ cannot possibly be divisible by a, and therefore the original hypothesis, viz., that $a^n = b^n + c^n$, must be impossible.

In order to show this, we will first observe that $(b+c)^n$ is greater than $b^n + c^n$, since it exceeds it by the quantity $n b^{n-1} c + \frac{n\,.\,\overline{n-1}}{2} b^{n-2} c^2$ + &c.

But $b^n + c^n = a^n$ by hypothesis.

$$\therefore\ \overline{b+c}|^n > a^n, \text{ and } b+c > a.$$

On the other hand, since evidently

$$b < a \text{ and } c < a,\ b+c < 2a.$$

But since $b+c$ is greater than a and less than $2a$, it cannot possibly be divisible by a. Which was to be shown. And it therefore follows that the equation $a^n = b^n + c^n$ is impossible, if n is an odd number > 1, always supposing, however, that a is a prime number.

Demonstration of Theorem II.

Let us suppose, if possible, that $a^n = b^n - c^n$. Then since $b^n - c^n$ is always divisible by $b-c$, let the quotient be Q. Therefore $a^n = \overline{b-c}\,.\,Q$. Now since a is a prime number, the first side of this equation is the product of the n factors $a \times a \times a \times$ &c.

Consequently the second side of the equation is the product of the same n factors, and it cannot possibly have any other. Therefore, since it has the factor $b-c$, this factor must itself be either $= a$ or divisible by a. But, on the other hand, it can be shown, as follows, that it is not divisible by a.

Since $a^n = b^n - c^n$, therefore $b^n = a^n + c^n$, and b is the greatest of the three numbers. Now since $\overline{a+c}|^n > a^n + c^n$, and $a^n + c^n = b^n$, $\therefore\ \overline{a+c}|^n > b^n$, and $a+c > b$, and $a > b-c$.

Since, therefore a is greater than $b-c$, it cannot possibly divide it. And therefore the original hypothesis that $a^n = b^n - c^n$ is impossible, if n is any number > 1, always, however, on the supposition that a is prime.

But in reviewing this demonstration, we find a case of exception; for it will be seen that we assert that $b-c$ can have no factor different from a. This is correct in one sense, but not so in another, since it may have the factor *unity*, which is usually disregarded, though it is here a consideration of the greatest importance. And since we have shown that $b-c$ cannot have either the factor a or any other factor, it follows that it can have no factor except *unity*, that is to say, it must be itself equal to unity, and can have no other value.

The above two theorems form together a conclusive demonstration of FERMAT's theorem, in the case of a prime number.

Extension of FERMAT'S *Theorem.*

Always supposing that a is a prime number, and that $b-c$ is greater than unity, the theorem $a^n=b^n-c^n$ (impossible), may be extended to a much more general theorem, viz., that $a^m=b^n-c^n$ is impossible, provided that m is less than n.

Demonstration. **Let $a^m=k^n$, where we no longer suppose k to be an integer. Therefore since $m < n$, a must be greater than k. But $k^n=b^n-c^n$ by hypothesis; therefore $b^n=c^n+k^n$, which is less than $\overline{c+k}|^n$; therefore b is less than $c+k$, and $b-c$ is less than k.** ***A fortiori,*** **$b-c$ is less than a.**

But in the given equation $a^m=b^n-c^n$, since a is prime, and $b-c$ divides b^n-c^n, therefore $b-c=a$, or else is divisible by a, a number which we have shown to be greater than itself, which is impossible. Therefore $a^m=b^n-c^n$ is impossible unless $m > n$. But if $m > n$, it is possible.

Example. **$3^3=6^2-3^2$, where a is prime and $b-c$ greater than 1, but $m > n$.**

By an analogous method we obtain the extended theorem No. II.

If n is an odd number, $a^m=b^n+c^n$ is impossible, provided that a is prime, and $m < n$.

We have hitherto supposed a to be prime, whereas FERMAT'S theorem has no such limitation; it remains, therefore, to enquire how far the present extended theorems are true when a is not a prime number.

In conclusion, we may oberve that the ancients themselves had discovered the possibility of the equation $a^2=b^2+c^2$.

But from what precedes, we may deduce the following theorems concerning it.

1. If $a^2=b^2+c^2$, and c is a prime number, then $a-b$ is always $=1$.

2. If $a^2=b^2+c^2$, b and c cannot *both* be prime numbers. For because c is prime, it follows that $b=a-1$.

And because b is prime, therefore $c=a-1$, therefore $b=c$, and $a^2=2\,b^2$.

But this is impossible, since one square cannot be double of another, in integer numbers.

Examples. **$5^2=4^2+3^2$, and 3 being prime, we have $5-4=1$.**

Again, $13^2=12^2+5^2$, and 5 being prime, we have $13-12=1$.

Again, $25^2=24^2+7^2$, and 7 being prime, we have $25-24=1$.

The converse, however, is not true. For if $a^2=b^2+c^2$, and $a-b=1$, it by no means follows that either b or c is a prime. For example, $221^2=220^2+21^2$, none of which numbers are primes.

XXVIII.—*On a Proposition in the Theory of Numbers.* By Balfour Stewart, Esq., of the Kew Observatory.

(Read 21st April 1856.)

Problem. If p be one of the roots of the equation $x^m-1=0$, (not 1,) then $(1-p)(1-p^2) \ldots . (1-p^{m-1})=m$, provided m is a prime number.

If m be not a prime number, and if $p_1=\cos\frac{2\pi}{m}+\sqrt{-1}\sin\frac{2\pi}{m}$, the same will hold for all roots $p=p_1^a$, where a is a number $<m$ and prime to m. But for all roots $p=p_1^a$, where a, or one of its prime factors, is also a prime factor of m, the product $(1-p)(1-p^2) \ldots . (1-p^{m-1})$ will be equal to 0.

Preliminary Propositions.

I. If m be a prime number, and a, β two numbers, each less than m; and if $a\beta=\gamma m+\delta$, where δ is less than m; then δ is neither equal to a nor to β.

For if $\delta=a$, we shall have $a(\beta-1)=\gamma m$, where a and $\beta-1$ are both less than m, which clearly violates the well-known theorem that a number cannot be made up in two ways of prime factors.

II. Again, if $a(\beta+\beta_1)=\gamma_1 m+\delta_1$ (where $\beta+\beta_1<m$); then δ_1 is not equal to δ.

For if $\delta_1=\delta$, then

$$\begin{aligned} a(\beta+\beta_1) &= \gamma_1 m+\delta \\ &= (\gamma_1-\gamma)m+\gamma m+\delta \\ &= (\gamma_1-\gamma)m+a\beta \end{aligned}$$

therefore $$a\beta_1=(\gamma_1-\gamma)m,$$

or a number is made up in two ways of prime factors, which is impossible.

III. If, therefore, we arrange in a horizontal row all the numbers in order of magnitude from 1 to $\overline{m-1}$ inclusive, and the same in a vertical row downwards, so that the two columns shall form the adjacent sides of a square, and if we multiply each successive number in the top horizontal column by the number in the vertical (as in the multiplication table), divide the product by m, and write down the remainders: then none of these remainders will be the same, for they will not be the multiplier itself (Prop. I.), nor, 2°, will any two be alike (Prop. II.). The multiplier and remainders will contain all the numbers from

1	2	3	4	5	6
2					
3	6	2	5	1	4
4	1	5	2	6	3
5					
6					

1 to $\overline{m-1}$ inclusive, though not in order of magnitude. We have exhibited this in the margin for $m=7$, the multipliers being 3 and 4.

IV. If m be not a prime, but composed of the prime factors $a \times b \times c \times d \times e \times f \times \ldots$, and if a or one of its prime factors (f) be also one of the prime factors of m, then in some case will $a\beta=\gamma m$, where β is one of the numbers 1, 2, 3, ... $\overline{m-1}$.

For $\frac{m}{f}$ and $\frac{a}{f}$ are whole numbers $< m$; and $a \,.\, \frac{m}{f}=\frac{a}{f} \,.\, m$, which satisfies the condition.

V. If neither a nor any of its prime factors is also a prime factor of m, then there will be a remainder $\delta < m$ whatever be β.

For if not, let $a\beta=\gamma m$; then since $m > \beta \therefore \gamma < a$. Therefore γ is either prime to a, or there is a factor in a which is not in γ: but this factor is, by hypothesis, not in m: it is, consequently, not in γm, which is absurd.

VI. The remainders will be different for every different value of β. For if possible, let

$$a\beta=\gamma m+\delta$$

$$a(\beta+\beta_1)=\gamma_1 m+\delta$$

$$=(\gamma_1-\gamma)\, m+a\beta$$

therefore, $a\beta_1=(\gamma_1-\gamma)\, m$ which, as in the last Prop., is impossible.

VII. If, then, we arrange the numbers as in Prop. III., we shall have 0 amongst the remainders, for all values of a which either divide m, or have a prime factor in common with it; whilst, for all other values of a, we shall have the same results as in Prop. III. This is shown for $m=8$ in the margin, for the multipliers 3, 4, and 6.

1	2	3	4	5	6	7
2						
3	6	1	4	7	2	5
4	0	4	0	4	0	4
5						
6	4	2	0	6	4	2
7						

Problem 1. If m be a prime number, the roots of the equation $x^m-1=0$ are $1, p_1, p_1^2, \ldots p^{m-1}$ where $p_1=\cos\frac{2\pi}{m}+\sqrt{-1}\sin\frac{2\pi}{m}$.

For, from the theory of equations,

$$x^m-1=(x-1)(x-p_1)(x-p_1^2)\ldots(x-p_1^{m-1})$$

or $$x^{m-1}+x^{m-2}\ldots+1=(x-p_1)(x-p_1^2)\ldots$$

putting 1 for x, $$m=(1-p_1)(1-p_1^2)\ldots(1-p_1^{m-1}).$$

If $p=p_1^a$, since $p_1^m=1$, $p_1^{\gamma m}=1$;

and if $a\beta=\gamma m=\delta$, $p_1^{a\beta}=p_1^{\delta}$

hence, $(1-p_1^{\alpha})(1-p_1^{2\alpha}) \ldots (1-p_1^{(m-1)\alpha})$ will be the same as

$$(1-p_1^{\delta_1})(1-p_1^{\delta_2}) \ldots (1-p_1^{\delta_{m-1}}),$$

where δ_1, δ_2, &c. are the remainders. But, by Prop. III., δ_1 δ_2, &c., are all different, and are the numbers 1, 2, . . . $\overline{m-1}$: hence,

$$\begin{aligned}(1-p)(1-p^2) \ldots (1-p^{m-1}) &= (1-p_1^{\alpha})(1-p_1^{2\alpha}) \ldots \\ &= (1-p_1)(1-p_1^2) \ldots (1-p_1^{m-1}) \\ &= m.\end{aligned}$$

Problem 2. If m be not a prime number, the same equation is true, by Prop. VII., for $p=p_1^{\alpha}$, where a is less than, and prime to m; but if a or one of its prime factors is a prime factor of m, then one of the factors $(1-p_1^{\alpha})(1-p_1^{2\alpha}) \ldots (1-p_1^{(m-1)\alpha})$ will be of the form $1-p_1^{\gamma m}=0$; and, consequently, the product itself will be equal to 0.

XXIX.—*On the Prismatic Spectra of the Flames of Compounds of Carbon and Hydrogen.* By William Swan, F.R.S.E.

(Read 21st April 1856.)

The phenomena presented by the prismatic spectra of flames have occupied the attention of many and excellent investigators. In most instances, however, no attempt has been made to procure accurate measurements of the positions of the bright lines which many of the spectra exhibit; and much in this field of observation, therefore, remains to be accomplished. I purpose, from time to time as I shall have leisure, to make a series of observations, whose object shall be the actual numerical determination of the positions of the bright lines in the spectra of flames; and I have commenced the series with an examination of the spectra of the flames of compounds of carbon and hydrogen. In an investigation into the phenomena of flames, the compounds of carbon and hydrogen claim our first attention, as constituting the most important means of artificial illumination; for it is scarcely necessary to remark, that, with the grand exception of sun-light, the combustion of these substances is the source of nearly all the light and heat from which we derive such extensive benefits in the arts and in domestic economy. It will be found, moreover, that the spectra of carbohydrogen flames possess, in common, remarkable features, which seem as yet to have received little attention, but which promise to be of service in explaining the general phenomena of artificial light.

If we examine the spectrum of the brightest part of the flame of an oil-lamp or a tallow-candle, it will be found that it exhibits no dark intervals, and that its colour and brightness vary gradually from point to point with scarcely any breach of continuity. If, however, we observe only the light proceeding from the blue part of the flame, which surrounds the upper part of the wick, a totally different result is obtained. The extreme red and violet rays become nearly or altogether invisible, and the intermediate portion of the spectrum exhibits a series of bright lines separated by dark intervals.* Similar lines occur in the flames of alcohol, sulphuric ether, and wood spirit. They are seen, however, with great difficulty in the flame of impure wood spirit, and are scarcely, if at all visible, in the more luminous flames of oil of turpentine and coal naphtha. Before offering an explanation of these differences, it will be necessary to premise some particulars regarding the nature of flame.

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* For descriptions of lines in the spectrum of an oil lamp flame, see Fraunhofer, Astronomische Abhandlungen, 1823, p. 16; Herschel, Edin. Trans., vol. ix., p. 455, and article Light, Encyc. Metrop., art. 522. The spectrum of the blowpipe cone is described by Fraunhofer, Brewster's Journal of Science, vol. vii., p. 7; and by Draper, Phil. Mag., vol. xxxii., p. 111.

On the Nature of Flame.

We owe to HOOKE, probably the first careful inquiry into the constitution of flame. More recently, the subject has been studied by Sir HUMPHRY DAVY and Professor DRAPER.*

The flame of coal gas or of a common candle, as is well known, consists of several portions, readily distinguishable by the eye, and in which the matter composing the flame exists in very different conditions. There is, first, the interior non-luminous portion, composed of gases not yet ignited; secondly, a blue conoidal shell, near the wick or burner, which, as it extends upwards, seems gradually to change its colour to a brilliant yellowish white; and, thirdly, an outer mantle or envelope of faintly luminous matter. On a careful examination, it will be found that the blue cone envelopes the white one; the blue, gradually thinning out towards the top, and the white, towards the bottom of the flame.

It has been supposed that, in the blue portion of the flame, the supply of oxygen is sufficient to insure the complete combustion of the gases, so that, in a carbohydrogen flame, there is the immediate production of water and carbonic acid.† The bright white light of the upper portions of the flame was proved by Sir HUMPHRY DAVY to proceed from the separation of solid carbon, which becomes brilliantly incandescent at the high temperature to which it is exposed, and which, when not converted into carbonic acid, escapes in the form of smoke.

The external mantle of the flame, according to Professor DRAPER, derives its light chiefly from incandescent carbonic acid and aqueous vapour.

While in the ordinary flames of coal gas and oil, solid carbon is separated, it is well known that by burning a mixture of gas and air, the separation of carbon may be entirely prevented, and a smokeless flame obtained. My attention was at first accidentally directed to the subject of this paper while using a species of gas lamp in which this object is effected in a very simple manner. As this lamp—the invention of Professor BUNSEN of Heidelberg—has only lately been introduced‡ into this country, and as I have made extensive use of it in my experiments, it may be proper to explain its construction.

It consists of a common "union" or "bat-wing" gas burner, which, when used in the ordinary manner, would produce a flat, fan shaped flame. The burner is surrounded by a brass tube, 0·4 inch in diameter, and about 3 inches in height, having apertures immediately below the burner, which can be opened or closed, so as to admit a regulated supply of air. The gas issuing from the burner in a fan shape strikes obliquely the walls of the tube, and being reflected from them, becomes effectually mixed with the air which enters at the bottom; and the mixture of gas and air, when lighted, burns at the top of the tube with a voluminous flame, without smoke. A thorough mingling of the gas and air is essential to the success of the arrangement, which will be found to fail when a plain burner is substituted for the "*union jet*."

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* Works of Sir H. DAVY, vol. vi., Lond. 1840. DRAPER on the Production of Light by Chemical Action, Lond. Phil. Mag., 1848, vol. xxxii., p. 100.

† KANE'S Chemistry, p. 289.

‡ By Dr ROBERT FERGUSON, whose interesting account of the lamp will, I believe, appear in the Transactions of the Royal Scottish Society of Arts.

The flame of the Bunsen lamp consists of at least two distinct portions,—a luminous hollow cone of a strong bluish green colour, about two inches in height, and a very diffuse outer mantle, about 6 inches in height, reddish towards the interior, but externally of a pale lavender tint. In this, also, as in the ordinary flame of coal gas, but in much greater profusion, there is a perpetual scintillation of yellow sparks, arising apparently from foreign matter suspended mechanically, partly in the gas, and probably more abundantly in the air which enters the tube and mingles with the gas before combustion. This matter is projected continually through the walls of the flame, where it becomes for a moment incandescent.*

The light of the exterior envelopes of flames, is, I conceive, chiefly due to the presence of minute particles of solid matter, derived partly from the substance undergoing combustion, and often, as in the case of the Bunsen lamp, from the air which enters the flame. The outer envelope of the flame of the Bunsen lamp possesses so little inherent luminosity, that it is peculiarly susceptible of having its colour influenced by the accidental presence of foreign matter. As the salts of sodium are well known to be remarkably energetic in producing homogeneous yellow light, I made the following experiment, in order to ascertain how small a portion of matter could in this way render its presence sensible.

One-tenth of a grain of common salt, carefully weighed in a balance indicating $\frac{1}{100}$ of a grain, was dissolved in 5000 grains of distilled water. Two perfectly similar slips of platinum foil were then carefully ignited by the Bunsen lamp, until they nearly ceased to tinge the flame with yellow light; for to obtain the total absence of yellow light is apparently impossible. One of the slips was dipped into the solution of salt, and the other into distilled water, the quantity of the solution of salt adhering to the slip, being considerably less than $\frac{1}{20}$ grain, and both slips were held over the lamp until the water had evaporated. They were then simultaneously introduced into opposite sides of the flame; when the slip which had been dipped into the solution of salt, invariably communicated to a considerable portion of the flame a bright yellow light, easily distinguishable from that caused by the slip which had been dipped into pure water. It is thus proved, that a portion of chloride of sodium, weighing less than $\frac{1}{1,000,000}$ of a grain is able to tinge a flame with bright yellow light; and as the equivalent weights of sodium and chlorine are 23 and 35·5, it follows, that a quantity of sodium not exceeding $\frac{1}{2,500,000}$ of a troy grain renders its presence in a flame sensible. If it were possible to obtain a flame free of yellow light, independently of that caused by the salt introduced in the experiment, it is obvious that a greatly more minute portion of sodium could be shown to alter appreciably the colour of the flame. It therefore follows, that much caution is necessary in referring the phenomena of the spectrum of a flame to the chemical constitution of the body undergoing combustion. For the brightest line in the spectrum of the flame of a candle,—the yellow line R* of FRAUNHOFER,—can be produced in great brilliancy, by placing an excessively small portion of salt in a flame, in whose spectrum that line is faint or altogether absent. The question then arises, whether this line in the candle flame is due to the combustion of the carbon and hydrogen of which tallow is chiefly composed, or is caused by the minute traces of chloride of sodium contained in most animal matter. When indeed we consider the almost universal diffusion of the salts of sodium, and the remarkable energy with which they produce yellow light, it seems highly probable that the yellow line R, which appears in the spectra of almost all flames, is in every case due to the presence of minute quantities of sodium.

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* If the air be *dusty* from any cause, these scintillations become very abundant. Thus, if the floor be swept, or a piece of charcoal be scraped with a knife at a little distance from the lamp, minute particles are carried by the current of air into the tube, and cause a profusion of sparks, which exhibit a very beautiful appearance, while they confirm the opinion that the ordinary sparks are occasioned chiefly by particles of dust carried by the air.

The view, which would attribute a great portion of the light of the envelopes of flames to the adventitious presence of minute traces of foreign matter, may possibly serve to explain certain anomalous diversities of colour which are observed in the envelopes of flames arising from the combustion of the same elements. Thus tallow, coal gas, anhydrous alcohol, and weak spirit of wine, all contain the same combustible substances, carbon and hydrogen: yet the envelope of the flame of a candle is bright yellow, that of a coal gas flame is purple, and those of strong alcohol and weak spirit differ greatly in luminosity.

It is important also to remark, that while the luminosity communicated to the exterior envelope of a flame by such substances as the salts of sodium or of copper, may be so great as to disguise that of the inner bright cone of the flame, or in some cases to render it altogether invisible; yet I have ascertained that the light of the blue portion of the flame, or of the inner cone, remains absolutely unchanged in colour and intensity. The proof of this curious property of flame will be given in the sequel.†

Prismatic Analysis of Flame.

Reserving, meantime, a more complete description of the apparatus I have employed, it may be sufficient to premise, that, in what I shall have to say regarding the spectra of flames, the object observed is supposed to be a narrow illuminated slit, viewed through a glass prism mounted before a telescope, which has been adjusted to focus on the slit.

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* SCHUMACHER's Astronomische Abhundlungen, 1823, p. 18.

† See p. 419.

It has already been stated, that certain carbohydrogen flames afford spectra exhibiting bright lines separated by dark spaces. In no spectrum are these lines more easily observed than in that of the Bunsen gas lamp. In order to distinguish the phenomena of the spectrum which are due to different portions of the flame, it is sufficient to place the lamp before a narrow vertical slit not exceeding 0·2 inch in *height*, through which its light passes to the prism.* If the lamp be gradually raised before the slit, the spectrum first seen will be derived exclusively from the envelope of the flame, which reaches high above the top of the interior cone. This spectrum is tolerably bright, extending without the least interruption, from the line C, nearly to the line H of FRAUNHOFER, and exhibiting no bright line whatever except the yellow line R. That line, however, is extremely flickering, so as often to disappear completely; and it seems due entirely to the yellow scintillations which abound in the exterior envelope. When the flame is raised still higher, so as to bring the top of the green cone into view, four other bright lines begin to appear; and as we continue to raise the flame so as to derive light from lower and lower portions of the flame, the bright lines become more and more clearly defined, owing to the intervening spaces becoming darker; and some fainter lines become visible. At length, when light passes through the slit only from the lowest portions of the flame where the exterior envelope nearly disappears, the bright lines become so sharply defined as to admit of their places being ascertained by actual measurement, with almost the same accuracy which is attainable in observations of the dark lines of the solar spectrum.

From the facility with which the lines of the carbohydrogen spectrum are obtained, I conceived they might be of use in optical researches; and I soon found them of great service in the prosecution of my experiments. I was, therefore, anxious to ascertain whether they belonged really to the gas flame, or were caused by the accidental presence of foreign matter; for it is well known that some metals, such as copper, when present in a flame, produce bright lines in its spectrum. For this purpose I burned a mixture of coal gas and air, successively, from an iron tube, a glass tube, a tube formed of a coil of platinum foil, and the brass tube of the Bunsen lamp; but in every case the lines remained unchanged in number and position, proving that they arose entirely from the combustion of the gas, and not from any matter derived from the lamp.

On the Apparent Diversity of the Spectra of Compounds of Carbon and Hydrogen.

Having thus studied the general phenomena of the spectra of carbohydrogen flames, some of which exhibit continuous, and others interrupted spectra, we may now resume the question, Whence do these differences arise?

It has been found by Professor DRAPER that an incandescent solid body emits light of every degree of refrangibility between limits varying with its temperature. Thus, when carbon burns in oxygen, or when a strip of platinum is heated, to a temperature of 2130° Fahr., by the passage of a current of electricity, a perfectly continuous spectrum is produced, without any bright lines or dark spaces, and extending at least from the line B to the line H of the solar spectrum.* This enables us to explain why we may see, in the spectrum formed by the blue portion of a flame, bright lines and dark spaces, which are totally invisible in the spectrum of the bright inner cone. For the light of the inner cone arises from incandescent solid carbon deposited in the flame, which, as we have just now seen, must produce a brilliant continuous spectrum. The light of this spectrum overpowers the comparatively faint illumination of the bright lines of the spectrum formed by the blue part of the flame, while it fills up the dark intervals between them; and both causes conspiring render the lines invisible. Again, lines may easily be seen in the spectrum of the blue part of a spirit lamp or candle flame, which fail to show themselves when we examine the flames of oil of turpentine or coal naphtha; for the latter bodies contain so much carbon that it begins to be deposited almost at the very bottom of the flame. The blue conoid is thus reduced to an extremely narrow ring; and it is practically impossible, however small the aperture through which light passes to the prism, to obtain the spectrum of the blue light separated from that of the incandescent carbon.

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* Or we may adopt Professor DRAPER's ingenious mode of observing flames through a *horizontal* slit, Phil. Mag., vol. xxxii. p. 106.

We can also similarly explain why lines may be visible in the spectrum of alcohol, which may not be easily seen in that of weak spirit of wine, or of impure wood spirit. The exterior envelope, like the interior bright cone, derives most of its light from incandescent solid matter, and produces a continuous spectrum, as was shown in the case of the Bunsen lamp. Now the exterior envelope of the flame of weak spirit of wine, or of wood spirit, is very voluminous and fully developed, and hence of unusual thickness near the bottom of the flame. The light derived from the incandescent matter it contains, will therefore operate precisely like that of the interior luminous cone, in rendering the bright lines of the spectrum invisible.

On Methods of Observing the Spectra of Carbohydrogen Flames.

Since the continuous spectra due to the light of incandescent matter offer no distinguishing features, it follows, that in searching for phenomena characteristic of the chemical constitution of bodies undergoing combustion, we must examine that part of the flame in which any solid molecules, which are being deposited, have not been able to collect into masses. This state of things exists in the blue portion of the carbohydrogen flames, where the supply of air is sufficient completely to consume the gases. In flames, such as that of oil of turpentine, $C_{10}H_8$, where there is much carbon, it becomes necessary to burn the carbon by an artificial supply of air. Two methods occurred to me of effecting this object, so as to convert the carbon into carbonic acid, without its intermediate separation in a solid form. One was to burn the vapours of the substances under examination in the Bunsen lamp; but this I rejected as inconvenient, and perhaps even in some cases dangerous, from the risk of explosion, where it would have been necessary to boil highly volatile liquids in close vessels. The other was simply to pass a stream of air through the flame by means of a table blowpipe. By means of the latter expedient I succeeded so completely in preventing the separation of solid carbon, as to obtain spectra with bright lines and dark spaces, in the case of every compound of carbon and hydrogen which I have as yet submitted to examination.

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* Phil. Mag., vol. xxx., p. 349.

Comparison of the Spectra of the Flames of various Substances containing Carbon and Hydrogen.

The hydrocarbon compounds which I have examined, and which are enumerated in the following table, may be divided into two classes; one consisting of substances containing only carbon and hydrogen, of which the general formula is $C_r H_s$, and the other of substances containing carbon, hydrogen, and oxygen, represented by the formula $C_r H_s O_t$.

$C_r H_s$

Light carburetted hydrogen,	$C H_2$
Olefiant gas,	$C_2 H_2$
Paraffin,	$C_{20} H_{20}$
Oil of turpentine,	$C_{10} H_8$

$C_r H_s O_t$

Methylic alcohol,	$C_2 H_4 O_2$
Alcohol,	$C_4 H_4 O_2$
Ether,	$C_4 H_5 O$
Methylic ether,	$C_2 H_3 O$
Glycerine,	$C_6 H_8 O_6$
Spermaceti,	$C_{64} H_{66} O_4$
Camphor,	$C_{10} H_8 O$
Wax,	Of indefinite composition.
Tallow,	
Coal gas,	
Coal naphtha,	

Of these substances, the light carburetted hydrogen was made by heating acetate of soda, hydrate of potassa, and quicklime; and the methylic ether from wood spirit and sulphuric acid. The gases were generally burned from a platinum jet, immediately after passing through a tube filled with pieces of quicklime. The glycerine, a substance which burns with difficulty, was heated in a platinum capsule; and the paraffin, camphor, and spermaceti, which were colourless, crystalline, and apparently pure specimens, were also similarly treated, in order to corroborate the conclusion stated at p. 415, that the lines observed in the spectra were all due to the combustion of the carbohydrogen compounds, and not to the presence of foreign matter. The alcohol, ether, and other liquids, were burned in lamps made of small phials,—a glass tube furnished with a cotton wick serving as a burner.

Taking the spectrum of the Bunsen lamp as a standard, the spectra of the other flames were compared with it, by viewing both simultaneously,—the light from the two flames passing through the same narrow slit.

The result of this comparison has been, that, in all the spectra produced by substances, either of the form $C_r H_s$ or of the form $C_r H_s O_t$, the bright lines have been identical. In some cases, indeed, certain of the very faint lines, which occur in the spectrum of the Bunsen lamp, were not seen. The brightness of the lines varies with the proportion of carbon to hydrogen in the substance which is burned, being greatest where there is most carbon. Thus, in the spectra of light carburetted hydrogen, pyroxylic spirit, and glycerine—substances which contain comparatively little carbon—certain of the fainter lines of the Bunsen lamp spectrum were not seen; but all those that *were* seen were identical with the lines of the coal-gas flame. I have no doubt that the fainter lines were really present, but were invisible, merely owing to their feeble luminosity; and this is rendered more probable by the fact that the number of lines visible in any spectrum varies with the brightness of the light. Thus in the solar spectrum, or in that of the Bunsen lamp, the fainter lines disappear when the intensity of the light is diminished.

The absolute identity which is thus shown to exist between the spectra of dissimilar carbohydrogen compounds is not a little remarkable. It proves, 1*st*, that the position of the lines in the spectrum does not vary with the proportion of carbon and hydrogen in the burning body; as when we compare the spectra of light carburetted hydrogen, $C H_2$, olefiant gas, $C_2 H_2$, and oil of turpentine, $C_{10} H_8$; and, 2*dly*, that the presence of oxygen does not alter the character of the spectrum; thus, ether, $C_4 H_5 O$, and wood spirit, $C_2 H_4 O_2$, give spectra which are identical with those of paraffin, $C_{20} H_{20}$, and oil of turpentine, $C_{10} H_8$.

In certain cases, at least, the mechanical admixture of other substances with the carbohydrogen compound does not affect the lines of the spectrum. Thus I have found that a mixture of alcohol and chloroform burns with a flame having a very luminous green envelope—an appearance characteristic of the presence of chlorine—and no lines are visible in the spectrum. When, however, the flame is urged by the blowpipe, the light of the envelope is diminished, and the ordinary lines of the hydrocarbon spectrum become visible.

Comparison of the Carbohydrogen and Solar Spectra.

Having ascertained, that probably all substances of the forms $C_r H_s$ and $C_r H_s O_t$ produce, when burning, spectra which are absolutely identical, I was desirous to compare their spectra with that of sun light.

For this purpose I at first attempted to view the solar spectrum and that of the Bunsen lamp simultaneously, but the great comparative faintness of the latter rendered that mode of comparison exceedingly difficult. I therefore determined to measure separately the minimum deviations for the principal lines of the solar and gas spectra; the intervals between the adjacent smaller lines of the latter spectrum being ascertained by means of a micrometer.

The instruments I employed were an excellent theodolite by Adie, and a very fine flint-glass prism by Secretan of Paris, whose faces have an area of four square inches, and which shows, with great distinctness, the finest lines in Fraunhofer's map of the solar spectrum. I am indebted for the use of these instruments to the kindness of Mr John Adie and Professor Forbes.

The prism being placed in its position of minimum deviation, the indices of refraction given in the sequel were calculated by the formula

$$\mu = \frac{\sin \frac{1}{2}(I + D)}{\sin \frac{1}{2} I};$$

where I is the angle of the prism, and D the deviation of the transmitted light.

I have denoted the five brightest lines of the carbohydrogen spectrum by the letters, $a, \beta, \gamma, \delta, \zeta$; and the fainter lines by which they are accompanied by $\beta_1, \beta_2, \gamma_1$, &c. In the tables, $D_A, D_\gamma, \mu_A, \mu_\gamma$, &c., denote respectively the minimum deviation of the rays, and the index of refraction for the lines A and γ of the solar and flame spectra.

A comparative diagram of the spectra of sunlight and the hydrocarbon flames is given in Plate VIII., fig. 1, where a is the double yellow line R of Fraunhofer. I have thought it advisable to introduce this line in the diagram, as it is almost constantly visible in ordinary artificial light, although, for reasons already fully stated, I conceive it is not peculiar to the spectra of carbohydrogen compounds. This conclusion is strongly corroborated by the remarkable phenomena pointed out at p. 414, namely, that the salts of sodium tinge the exterior envelope of the Bunsen lamp flame with so brilliant a yellow light, as completely to overpower the comparatively feeble blue light of the inner cone, and to render it altogether invisible; while yet the light of that portion of the flame remains absolutely unchanged. This remarkable property of flame is easily demonstrated by holding a slip of platinum, with some salt placed on it, in the flame, while the spectrum is observed through a telescope. The instant the salt reaches the flame, the yellow line R or a, which before may have been extremely faint, or altogether imperceptible, shines out in great brilliancy; while the lines β, γ, δ, and ζ remain totally unchanged in position, colour, and intensity.

While the line α is thus exceedingly variable in its brightness, the lines β, γ, δ, and ζ, on the other hand, are perfectly steady; and being never absent in carbohydrogen spectra, there is every reason to believe that they are really characteristic of the body undergoing combustion. Beyond α on the less refracted side there is a faint trace of red light, which, as it becomes so feeble as almost to disappear when the light is derived from the lowest point of the flame of the Bunsen lamp, is probably due to the exterior envelope of the flame, and not to the interior cone. The line α is separated from β by an extremely dark space, almost destitute of light. The line β is of a faint yellowish green colour, but well defined, and is accompanied by four almost equidistant lines β, β_2, &c., which diminish in brightness as their distance from β increases. After another very dark interval, the extremely beautiful line γ follows, which is exceedingly brilliant, and of such absolutely definite refrangibility as, like α, to form a perfectly sharp image of the slit through which the light passes. Its colour is a fine slightly bluish or pea green, and it is accompanied by a fainter line γ_1. The next line δ is the less refracted edge of a broad band of light containing four fine lines. This group, which is of a pale ashy colour, is separated by dark intervals from γ and ζ. The line ζ belongs to a brilliant but not very well defined band of a fine purple tint, which is accompanied by a fainter line ϵ.

I have completed observations of the minimum deviations for the lines α, β, γ, δ, and ζ; and also for the principal lines of the solar spectrum, which are given in Series 1, Tables II. and III., pp. 427, 428. From an examination of these tables it appears, that while several lines in the carbohydrogen spectrum coincide nearly in position with remarkable lines in the solar spectrum; yet in no case, if we except the line α, has the observed coincidence been exact. The observations, therefore, rather tend to prove that the *bright lines* of the carbohydrogen spectrum coincide, not with the *dark lines*, but with the *bright spaces* of the spectrum of sun light.

Postscript added since the preceding Paper was read.[*]

From the well known coincidence discovered by FRAUNHOFER, to exist between the line R, in the spectrum of a lamp, and D of the solar spectrum, taken in connection with similar phenomena, which have since been observed, it might be inferred, as a general law of the spectra of flames, that their bright lines always coincide with dark lines of the solar spectrum.

The result of the investigations which have now been detailed, is obviously unfavourable to such a conclusion. In publishing observations bearing on a question of so much interest and importance, I was anxious, if possible, to leave no doubt as to their accuracy; and since the preceding paper was read, I have made a much more extensive series of experiments, than the limited time I can devote to such researches, had then enabled me to overtake, involving the determination, with more or less accuracy, of the positions of all the bright lines in the carbo-hydrogen spectrum, whose presence I have been able to detect. These experiments, with some account of my methods of observation, I have deemed it desirable to append to the preceding paper.

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[*] Printed by permission of the Council.

Methods of Observation.

The theodolite A, fig. 2, which was used in measuring the deviations of the refracted rays, has a limb 7·5 inches in diameter, with two verniers reading 10″, and a telescope B, of 1·6 inch aperture, furnished with a parallel wire micrometer. The stage carrying the prism P, furnished with screws to render its faces perpendicular to the divided circle, was mounted over the centre of the theodolite; and, in order to avoid parallax, the object viewed was an extremely narrow slit placed in the principal focus of the object glass of a 30 inch telescope C L, which thus acted as a collimator.* The telescope rested in Ys, in a solid cast-iron stand, D D, which also carried the theodolite: so that the collimator preserved an invariable position in relation to the theodolite, notwithstanding any instability of the floor of the room in which the observations were made; and the zero of the circle was found to remain exceedingly constant. A diaphragm with a vertical slit was placed before the collimator lens, so as to limit the aperture in the plane of refraction to 0·4 inch, and thus to allow only a nearly central pencil of rays to fall on the prism. Any errors, which might have arisen, either from imperfect adjustment of the collimator to sidereal focus, or from defective aplanatism in its lens, were thus avoided as much as possible.

The deviations of the refracted rays were observed first to the right, and then to the left,—the prism being always adjusted to its position of minimum deviation,—so that the difference of the readings of the verniers in the two positions of the prism, gave double the minimum deviation of the refracted rays.

The angle of the prism was ascertained, by first turning it with its edge towards the object glass of the telescope, as represented in fig. 3, where ABC is the prism, and T the telescope. The stage carrying the prism was rigidly connected with the telescope, so that when the telescope was moved, the prism moved along with it; and being left undisturbed, the inclinations of its faces AB, AC, to the line of collimation of the telescope remained invariable. The telescope was then turned, until the image of the illuminated slit of the collimator, seen by reflection, successively in the two faces of the prism, was made to coincide with the telescope wires; and, at each intersection, the verniers were read off. The difference of the readings then gave double the angle of the prism. For, if DGI, FHK represent the course of the reflected rays, since the telescope has been adjusted to sidereal focus, GI and HK, must be parallel; and the angle DEF, will obviously be double the angle BAC. Now, since DE and EF, are at the times of observation successively in the *same* direction, namely, that of the parallel rays emerging from the collimator, it follows that the telescope must have been turned through the angle DEF. Hence the difference of the readings is DEF, or twice BAC.

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* I have described this mode of observation in my paper on the Ordinary Refraction of Iceland Spar, Edin. Trans., vol. xvi.

In order to test the adjustment of the collimator to sidereal focus, I made two series of observations of the angle of the prism given in Table I.; Series I. having been made by means of the collimator, and Series II. on a definite point of the tower of St Stephen's Church, distant about 2240 feet, where the parallax due to any difference in the directions of the rays incident on the two faces of the prism could not have caused an error exceeding 4″ in the measured angle.

These results agree so closely as to show that any want of parallelism in the rays emerging from the collimator, arising from want of perfect adjustment to sidereal focus, could not have appreciably affected the observations of the absolute deviations of the refracted rays. I may also observe, that since, during the observations of the carbohydrogen and solar spectra, the whole apparatus remained unaltered, any want of parallelism in the rays incident on the prism, whether arising accidentally from imperfect adjustment of the collimator, or necessarily from the unavoidable want of perfect achromatism in its lens,—for either cause might modify the apparent direction of the observed object, if the pencil of rays incident on the prism were not accurately *central*,—would affect the observed deviations in the two spectra alike. The accuracy of the observations, viewed merely as affording a comparative view of the relative positions in the scale of refrangibility occupied by the lines in the two spectra, would thus remain entirely unimpaired.

I have ascertained, however, by actual experiment, that the observations of *absolute* deviation cannot have been sensibly affected by any want of achromatism in the lens of the collimator. Having caused the telescope wires to coincide accurately with the image of the collimator slit, I illuminated the slit alternately with the extreme red and the extreme violet rays of the solar spectrum formed by a flint-glass prism. I then found that the image of the slit did not in the slightest perceptible degree alter its apparent position; so that, while the illumination was changed from red to violet light, the wires continued to bisect the slit with perfect accuracy.

As the spectrum of the Bunsen lamp is so faint that the telescope wires, when projected on all but its brightest lines, are invisible, it became necessary to illuminate the wires; but I speedily found that, from the feeble luminosity of the spectrum, observations with an illuminated field were nearly impracticable, and I was therefore obliged to observe with illuminated wires on a dark field. The arrangement for illuminating the wires which I devised is so simple, and proved so successful, that I venture to describe it, in the hope that it may prove useful in similar researches. A hole, *a* (see fig. 4), 0·1 inch in diameter, was drilled in the side of the tube in which the eye piece slides, at a point between the field lens of the eye piece and the wires *w*; a small lamp, L, furnished with a condensing lens *c*, and a conical tube with a small aperture *e*, through which alone light was allowed to pass, was attached to the telescope, so that the light, indicated by *r r*, emerging from the conical tube, and entering the hole in the eye piece tube, crossed the axis of the telescope at an angle of about 70°, so as to illuminate the intersection of the wires at *w*, on the side *next* the eye, while all the rest of the field remained perfectly dark. By slightly varying the position of the lamp, the illumination of the wires could be adjusted with the utmost nicety to suit the brightness of that portion of the spectrum which was under examination.

Notwithstanding the most careful adjustment of the illumination of the wires, I still found the observation of the fainter lines of the carbohydrogen spectrum extremely difficult. The brightness of the lines in the spectrum of the Bunsen lamp is, however, considerably augmented by urging the flame by the blowpipe; and I found it useful to employ three jets placed one behind another, so that the combined illumination of three blowpipe cones might fall upon the prism. This apparatus, which is useful in exhibiting the fainter lines of the carbohydrogen spectrum, is easily constructed by forming three blowpipe jets of glass tube, about 0·2 inch in diameter, in the ordinary manner, and placing them, side by side, in a perforated cork. The cork is then inserted in a short piece of wide tube, having at its other end a second cork, connected with a flexible tube conveying a current of air from a table blowpipe.

I have also carefully compared, by simultaneous observations, the spectrum of the Bunsen lamp flame urged by a jet of oxygen gas, with the spectrum obtained by means of the triple air blowpipe. The lines in the two spectra were almost equally bright, and differed neither in number nor in position.

In the observations, Series I., Tables II. and III., I used an eye piece giving a magnifying power of 11, which was afterwards superseded by another magnifying 21 times, with which Series II. was made.

Comparison of the Carbohydrogen and Solar Spectra.

The second series of observations having been made with a higher magnifying power, and in some other respects also in more favourable circumstances than the first, is to be regarded as more trustworthy; yet the results of both agree so closely, that any additional accuracy which might have been obtained by ascertaining, separately, the probable errors of the two series, and their most probable result, when combined, could scarcely have repaid the labour of the necessary computation. I have, therefore, deemed it sufficient to give all the observations equal weight, and to take simply the arithmetical mean of the whole. The mean results of the two series, and the number of observations in each, being tabulated separately, the reader will be able to form some judgment regarding the probable accuracy of the final determinations obtained from the two series combined. In Fig. 1, which is a graphic construction of the observations in Tables II. and III., the lines were drawn by the engraver through points laid down by me on the copper to a scale,—adopted to suit the size of the plate,—of one inch to 2200″. I have ascertained the errors in the positions of these lines to amount, in one case only to ·01 inch (corresponding to 22″), and to be generally much less; so that the spectra are represented in the figure with tolerable fidelity.

In addition to the observations of the carbohydrogen and solar spectra contained in Table II., where the deviation for each line of either spectrum was separately determined by the theodolite or micrometer, I have also made simultaneous observations of the spectra of sun light and of olefiant gas. The gas, which was prepared by heating alcohol with sulphuric acid, was conducted through wash bottles containing caustic potash and sulphuric acid, to a gas holder; from which it afterwards passed, through a tube filled with pieces of quicklime, to a platinum jet where it was burned.

The lines in the spectrum of olefiant gas are very distinct, being well seen without using the blowpipe; but like the lines in the other carbohydrogen spectra, they are not sufficiently luminous to be seen when projected on the solar spectrum, unless the latter is made so faint, that its lines have disappeared. I succeeded, however, in observing the spectra simultaneously, by intercepting the sun light which fell upon one half of a narrow slit, and illuminating the whole slit with the flame of olefiant gas. The gas spectrum then appeared immediately over that of the sun, and the *brighter* lines in it were well seen, especially when the flame was urged by the blowpipe. The intervals between the lines of the gas spectrum and the nearest lines of the solar spectrum, given in Table V., were measured by the micrometer, with a magnifying power of 21; and the observations for the brighter line β, γ, and δ, agree well with those of Table III.

The line α was rarely visible in the spectrum of olefiant gas, and its appearance was only momentary, which confirms the opinion already stated, that it does not properly belong to the carbohydrogen spectra. To the proof already adduced in support of this opinion, I may here also add, that I have found it permanently absent in the flames of carbonic oxide, and of light carburetted hydrogen.* The continued invisibility of so brilliant a line of the spectrum, coupled with its almost instantaneous appearance at very long intervals,—for it did occasionally appear for a moment,—satisfactorily proved it to be due merely to foreign matter which had accidentally entered the flame.

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* I have found that the column of heated air rising from the flame of a spirit lamp with a salted wick, is most energetic in communicating yellow light to the exterior envelope of the flame of the Bunsen lamp. This effect is apparently confined to the outer, or oxidizing portion of the flame, where there is no excess of hydrogen, to decompose the chloride of sodium; and the experiment is interesting, as tending to prove that the yellow light may be caused by simple *incandescence*, without the actual *combustion* of sodium.

From an examination, either of Table IV., or of Plate VIII., fig. 1, it will be seen that certain of the lines in the carbohydrogen spectrum occupy nearly the same places in the scale of refrangibility with dark lines in the solar spectrum. These are the lines α, γ, δ_3, and ζ, which coincide more or less exactly with the lines D, b_2, F_1, and G. The first of these coincidences has been long known, having been discovered by FRAUNHOFER;[*] and similar remarkable relations have been observed by Sir DAVID BREWSTER to exist between certain lines in the spectrum produced by "deflagrating nitre," and the corresponding lines of the solar spectrum.[†]

From these singular coincidences occurring in so many different cases, the inference might be drawn, that *all* bright lines in the spectra of flames coincide with dark lines in the solar spectrum; and the extremely close proximity of the lines γ and b_2, δ_3 and F_1, ζ and G indicated in Table IV., might at first sight seem to confirm such an opinion. For it might be argued, that so close agreements in the ascertained deviations indicate absolute identity; the minute differences observed being attributed simply to errors in the observations. It will be seen, however, that the observed deviations of the lines b_2 and γ, differ by no less a quantity than 40″, which is quite beyond the sum of the probable limits of error in the observations for these lines, which I have ascertained to be only about 5″; and thus their coincidence is shown to be highly improbable.

But any remaining doubt on the subject is completely removed, by the simultaneous observations of the spectra of sun light and olefiant gas, given in Table V., where the micrometrical measurement of the interval between the lines b and γ differs only by 11″ from that obtained by the theodolite observations. In fact, the bright line γ was *seen* when the spectra were viewed simultaneously, to coincide, not with the dark line b_2, but with the clear space immediately beyond it. If we omit the line α, which, for reasons already fully stated, I do not regard as properly belonging to the carbohydrogen spectrum, not one of the other twelve lines which I have observed in that spectrum occurs near any conspicuous dark line of the solar spectrum, with the exception of the lines γ, δ_3 and ζ, which fall near b_2, F_1 and G. Now, of these, γ has been proved beyond doubt, not to coincide with b_2, but with a bright space in its vicinity; and from the simultaneous observation of the spectra of sun light and of olefiant gas, as well as from the results of the theodolite observations, I believe that the other *bright lines* of the carbohydrogen spectrum also coincide not with *dark lines*, but with *bright spaces* in the solar spectrum.

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[*] SCHUMACHER'S Astronomische Abhandlungen, 1823, p. 29. See also BREWSTER'S Edinburgh Journal of Science, vol. viii., p. 7. M. FOUCAULT has lately verified this result with the double yellow line seen in the spectrum of the voltaic arc, between charcoal electrodes. See DE LA RIVE'S Electricity, vol. ii., p. 322.

[†] Report of British Association, 1842, p. 15.

From the fact just stated, that most of the lines in the carbohydrogen spectrum occupy positions where, in the solar spectrum, no conspicuous dark lines occur, direct comparison of the spectra, by simultaneous observation, seems almost impossible; for, before the fainter lines of the carbohydrogen spectrum become visible, the solar spectrum must be rendered so faint, that its finer lines have disappeared. On the other hand, to make a complete comparison of the spectra by actually measuring with the theodolite, the positions of the finest lines of the solar spectrum, would be a most formidable task. For when we consider that FRAUNHOFER has represented on his map of the solar spectrum, 350 lines, while Sir DAVID BREWSTER, by the aid of very excellent optical means, has observed the spectrum to be "divided into more than 2000 visible and easily recognized portions, separated from each other by lines more or less marked,"[*] it follows, allowing 5° for the angular dispersion of the extreme rays of the spectrum,—that the average interval between the lines observed by him is only $9''$. Extremely delicate theodolite measurements would therefore be required, in order to determine, whether or not any bright line of a flame spectrum was or was not coincident with one or other of the numerous small lines of the solar spectrum; and even where a coincidence was ascertained, it might be fairly attributed to chance, just as a binary star may be only optically, and not necessarily physically double.

In cases, however, where there is a remarkable analogous configuration of two groups of lines, accompanied by exact coincidence, as between the double lines a and D; and more especially where we actually view the striking phenomenon of the lines in the spectra optically superimposed, the impression of some physical connection between the two groups becomes irresistible.

The coincidence of γ, the most brilliant line of the carbohydrogen spectrum, with the clear space immediately beyond b_2,—the most refracted line of a group, which, whether we regard the singular configuration or the strength of the lines which compose it, is perhaps the most notable in the solar spectrum,—is a phenomenon which seems deserving of attention as probably indicating also some physical relation.

In conclusion, I may observe, that from the facility with which, by means of the Bunsen lamp, the carbohydrogen spectrum may be obtained, and from the definite and readily identifiable character of the lines which compose it, these lines may be useful in optical researches, where, from any cause, sun light cannot be employed. It will be seen, from Table IV., that, for most practical purposes, the lines a, γ, δ_3, and ζ, may be assumed as identical with D, b_2, F_1, and G of the solar spectrum; any error in the index of refraction calculated on that assumption, affecting only the fourth or fifth place of decimals.—*7th June.*

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[*] Edinburgh Trans., vol. xii., p. 528.

TABLE I. *Observations of the Angle of the Prism.*

SERIES I.		SERIES II.		Total Number of Observations.	Mean of all the Observations.
Number of Observations.	Mean of the Observations.	Number of Observations.	Mean of the Observations.		
5	60° 0′ 15″	3	60° 0′ 18″	8	60° 0′ 16″

TABLE II. *Observations of the Solar Spectrum.*

Line of Spectrum.	SERIES I.		SERIES II.		Difference of Means I. and II.	COLLECTED OBSERVATIONS OF MINIMUM DEVIATIONS.	
	Number of Observations.	Mean I.	Number of Observations.	Mean II.		Total Number of Observations.	Mean of all the Observations.
A	3	47° 20′ 27″	3	47° 20′ 21″	0′ 6″	6	47° 20′ 24″
a	2	47 29 54	4	47 30 2	0 8	6	47 29 59
B	2	47 39 52	3	47 39 51	0 1	5	47 39 51
C	2	47 50 22	3	47 50 10	0 12	5	47 50 12
C_1*	...	...	1	48 0 50	...	1	48 0 50
D	2	48 18 15	4	48 18 12	0 3	6	48 18 13
E	2	48 55 24	4	48 55 26	0 2	6	48 55 25
b†	4	49 1 45	5	49 1 44	0 1	9	49 1 45
b_1	2	49 2 37	1	49 2 35	0 2	3	49 2 36
b_2	2	49 2 55	3	49 2 56	0 1	5	49 2 55
F	2	49 29 9	3	49 29 19	0 10	5	49 29 15
F_1*	...	...	4	49 49 2	...	4	49 49 2
G	2	50 35 2	3	50 35 5	0 3	5	50 35 4
H	1	51 34 50	6	51 34 43	0 7	7	51 34 44
	Mean Temp. of Prism, 53·3 F. Mean Barom. . . 30·21		Mean Temp. of Prism, 64·2 F. Mean Temp. of Air, . 62·5 Mean Barom. . . . 29·98				

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* C_1 and F_1 are used here to denote very strong lines adjacent to FRAUNHOFER'S lines, C and F.

† FRAUNHOFER denotes by *b* the two most refracted lines of a remarkable group, represented by three strong lines in his map of the solar spectrum. I have here denoted these lines by *b*, b_1, b_2, in the order of their refrangibility. On 20th May, about 7ʰ 10ᵐ P.M., when the sun was rather low in the horizon, but free from clouds, I observed with a power of 21, the line b_2 to be very finely but distinctly double, so that the group consists of *four* lines.

TABLE III. *Observations of the Carbohydrogen Spectrum.*

Line of the Spectrum.	SERIES I.		SERIES II.		Difference of Means I. and II.	COLLECTED OBSERVATIONS OF MINIMUM DEVIATIONS.	
	Number of Observations.	Mean I.	Number of Observations.	Mean II.		Total Number of Observations.	Mean of all the Observations.
α	5	48° 18′ 14″	2	48° 18′ 13″	0′ 1″	7	48° 18′ 14″
β	3	48 32 9	6	48 32 7	0 2	9	48 32 7
β_1	...	...	9	48 35 12		9	48 35 12
β_2	...	...	9	48 37 50		9	48 37 50
β_3	...	...	9	48 40 10		9	48 40 10
γ	9	49 3 35	4	49 3 31	0 4	13	49 3 34
γ_1	...	...	9	49 6 23			
δ	2	49 41 58	6	49 41 58	0 0	8	49 41 58
δ_1	...	...	9	49 44 48		9	49 44 8
δ_2	...	...	9	49 46 37		9	49 46 37
δ_3	...	...	9	49 48 41		9	49 48 41
ϵ	...	...	4	50 27 54		4	50 27 54
ζ	3	50 35 37	6	50 35 28	0 9	9	50 35 33
	Mean Temp. of Prism, 53° F. Mean Barom., . . . 30·02		Mean Temp. of Prism. 62·6 F Mean Temp. of Air, 60·4 Mean Barom. . . 29·56				

In Table III., the deviations of the lines α, β, γ, δ, and ζ, were alone determined by the theodolite; the other lines were then referred to α, β, &c., by micrometer observations.

TABLE IV. *Comparison of Contiguous Lines in the Solar and Carbohydrogen Spectra.*

SOLAR SPECTRUM.			CARBOHYDROGEN SPECTRUM.						
Line.	Deviation (D.)	Index of Refraction (μ.)	Line.	Deviation (D.)	Index of Refraction (μ.)				
D	48° 18′ 13″	1·621079	α	48° 18′ 14″	1·621083	$D_\alpha - D_D$	0′ 1″	$\mu_\alpha - \mu$	0·000004
b_2	49 2 55	1·628659	γ	49 3 34	1·628769	$D_\gamma - D_{b_2}$	0 39	$\mu_\gamma - \mu_{b_2}$	0·000110
F_1	49 49 2	1·636407	δ_3	49 48 41	1·636349	$D_{F_1} - D_{\delta_3}$	0 21	$\mu_F - \mu_{\delta_3}$	0·000058
G	50 35 4	1·644068	ζ	50 35 28	1·644147	$D_\zeta - D_G$	0 24	$\mu_\zeta - \mu_G$	0·000079

TABLE V. *Observations on the Spectrum of Olefiant Gas.*

Minimum Deviations for Lines in Solar Spectrum, from Table II.				Intervals between Lines in the Solar and Gas Spectra, measured by the Micrometer.					Minimum Deviations.		
					No. of Observations.						
D_D	48°	18′	13″	$D_\beta - D_D$	4	14′	10″	D_β	48°	32′	23″
				$D_{\beta_1} - D_D$	4	17	10	D_{β_1}	48	35	23
				$D_{\beta_2} - D_D$	3	19	58	D_{β_2}	48	38	24
				$D_{\beta_3} - D_D$	3	22	37	D_{β_3}	48	40	50
D_b	49	1	45	$D_\gamma - D_b$	3	1	38	D_γ	49	3	23
				$D_{\gamma_1} - D_b$	3	4	33	D_{γ_1}	49	6	18
D_F	49	29	15	$D_\delta - D_F$	3	12	44	D_δ	49	41	59
				$D_{\delta_1} - D_F$	1	20	29	D_{δ_1}	49	49	44

XXX.—*On the Laws of Structure of the more Disturbed Zones of the Earth's Crust.*

By Professor H. D. Rogers.

(Read 21st April 1856.)

Having, several years ago, in the course of a prolonged investigation of the geological structure of the Appalachian chain of the United States, conducted partly in co-operation with my brother, Professor W. B. Rogers, as a purely scientific inquiry, partly by myself, in connection with a Government Survey of the State of Pennsylvania, discovered what we deemed important laws, applicable generally to all corrugated tracts of strata; and being prepared, by observations since made in the United States and in Europe, to extend their application, and give them a more general expression, I have thought that I could not select a more suitable subject for my first communication to the Royal Society of Edinburgh, than this portion of descriptive and dynamic geology, which has engaged much of my attention, theoretically and practically, for these many years. In presenting an outline of the views already arrived at, and published by us as a necessary part of the further generalizations since reached, I will refrain from repeating, in historical detail, what we have already written, but will give our conclusions in the form and with the brevity most compatible with clearness, referring to the printed papers and communications where the special topics included in this more general summary may be seen.

Wave-like form of all Upraised Tracts of the Crust.

The first or most general fact which I would enunciate respecting any portion of the earth's crust that has suffered elevation or depression from the position or level in which its strata were originally deposited, is, that the displaced beds present invariably the form of one or many waves, even when within limited geographical areas they may seem to retain an approximate horizontality. This comprehensive statement respecting the wave-like structure of the earth's crust, is not invalidated by the instances of disordered dip seen in certain dislocated regions, such as some of the coal-fields of Great Britain; for it will generally be found that the breaks or faults in the strata only separate disarranged portions of what were originally continuous undulations.

In all large stratified areas, where the dip is both gentle and persistent in its direction throughout considerable spaces, and where this dip is genuine, the result, that is to say, of a true displacement of the mass, and not a consequence of the original obliquity of deposition called false bedding, the crust waves will be found to be of an amplitude proportioned to their flatness; but in those districts where the prevailing inclinations are steep, and where they are directed to opposite points, it will be found invariably that the inclined masses are but the parts of successive arches, or rather waves, the denuded or broken crests of which approach each other the closer as the dips are steeper.

Parallelism of the Crust Undulations.

It is, therefore, another general fact regarding disturbed zones of the crust, that where the displacement from horizontality has been great, the strata are arranged in longitudinal tracts, or great belts of parallel waves. These, where their symmetry of structure is not marred by dislocations of the crust, or hid by overlapping superficial deposits, exhibit a remarkable and beautiful resemblance to those great and continuous billows which are called by seamen rollers, and by mechanicians *waves of translation.* Far more continuous in their crests, more strictly parallel, and more symmetrical in form than the wind-produced waves upon the waters of the globe, such great swells or rolling billows, engendered by wholly different forces, are, I conceive, the true archetypes of the undulations visible in the more corrugated portions of the earth's crust. Perhaps in no uplifted district of the surface are these crust-waves so symmetrically developed, or so readily recognised, as in the Appalachian Mountains of the United States. It was there that WILLIAM B. ROGERS and myself, analyzing their forms, and tracing and connecting their axes, detected those phenomena of shape and gradation which led us to the general laws of crust flexures which we have ventured to publish.

But we believe that all mountain zones, and all corrugated districts generally, which have been elevated, like the Appalachians, at one epoch, and by crust movements observing only one prevailing direction, will be found to possess this wave-like structure, under similar conditions of gradation, and in a like conspicuous manner. It is only those tracts which have been revisited several times by the elevating and undulating forces, and especially those where the successive disturbances have not coincided in direction, but have crossed each other, causing interference and intersection of the waves, as in what is called a chopped sea,—such districts, for example, as the Swiss Alps and the mountains of Cumberland and Wales,—that we fail readily to discern the wave structure of the strata, or, perceiving it in part, are unable, without extreme toil and patience, to connect the originally related outcrops of the rocks, and reconstruct in our minds, and represent to the eye, the undulations that actually exist in a broken and disguised condition.

Wherever we have been led, either from observations in the field, or from a careful perusal of the descriptions of geologists, to a clear recognition of the dip-structure of any corrugated zone, whether mountain chain or otherwise, not confused by different systems of elevatory movements of the crust, we have become impressed with its marked resemblance in all the essential features of the undulations, both as respects the typical forms of the individual waves, and the grouping and gradation of the several sets of waves, to the flexures characteristic of the Appalachian chain of America. I was particularly convinced of this resemblance upon examining, in the summer of 1848, the structure of the Jura chain of Switzerland;* and scarcely less struck with the agreement I noticed between the phenomena on the borders of the Alps, especially in the Bernese Oberland, and the features which distinguish the most corrugated tracts at the south-eastern base of the chain of the Appalachians.

Relations of Flexures to each other.

If we regard now the flexures which constitute any great undulated or corrugated belt of strata, we shall find that these display the following laws or general facts of relationship:—

Parallelism.

1. When seen in their simplicity, or undisguised by cross breaks and undulations, those of a particular district show a remarkable degree of mutual parallelism. Not only are they parallel to each other, but to the general trend of the portion of the mountain system to which they belong, and especially to its chief igneous axes, where it possesses such.

2. The flexures or waves, where the undulated zones are wide and complex, occur in groups or lesser belts; those constituting such subordinate series observing the law of parallelism still more strictly than group does towards group. This remarkable parallelism of the adjacent flexures in an undulated region belongs not only to those waves and groups of waves which are rectilinear in their crests, but to such as curve even very considerably in their lineation. Nowhere, perhaps, is the constancy of this law so well displayed as in the Appalachians. This great mountain zone of the United States and Canada, about 1500 miles in length, and more than 150 in its maximum breadth, consists longitudinally of eleven different sections, *six* of which are straight, *three* curvilinear, and convex towards the north-west, and *two* also curvilinear, but convex towards the south-east. Three of the straight sections have an approximately east and west trend, and the other three an approximately north and south course. Notwithstanding the great windings in the direction of the chain thus indicated, it is remarkable that each division or segment of it, whether straight or curved, is made up of crust-waves and groups of waves, which are essentially in mutual parallelism; and wherever a seeming exception to this rule presents itself, as on the Upper Juniata in Penn-

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* See Abstract of Communication to American Association for Advancement of Science, Cambridge, Mass., March 1849, p. 113.

sylvania, and in Northern Vermont, it will be found to arise from the interference or interlocking of the ends of the waves of different but adjacent segments.

3. Crossing any great belt of anticlinal and synclinal flexures, such as that of the Appalachians, or that of Belgium and the Rhenish Provinces, it will be noticed, when the undulations are carefully traced and compared, that these consist of more than one class as respects dimensions; indeed they will be found to be of two or three grades, when grouped according to their length, height, and amplitude. In most parts of the Appalachian chain, there are at least two prevailing magnitudes in the waves. The chief class, or primary undulations, are of great size, their length amounting to from 50 to 120 miles, and their breadth to several miles, except where they are closely compressed. The subordinate or secondary waves are seldom more than a fourth of a mile wide, nor do they usually exceed ten miles in length, and in many groups they are much shorter. Frequently a third class is to be met with, of still smaller and less persistent flexures,—rolls of the strata, as they are called in the coal-mining districts of Pennsylvania,—which seem to be only local corrugations of the more superficial rocks, and not true undulations of the crust pervading the entire thickness of the formations. The relations of the primary to the secondary waves will be enlarged upon hereafter. It will suffice, under the present head of parallelism of flexures, to state that, for the Appalachians at least, those of the second order are not necessarily parallel to those of the first, though within a given district they observe among themselves the same mutual parallelism which the larger or primary waves exhibit.

FORMS OF THE WAVES.

Symmetrical Flexures.

The individual waves or flexures of a belt of undulated strata occur under three essential varieties of form. The first, or most simple, is that of a convex or concave wave, or in technical geological language, an anticlinal or synclinal flexure, in which the two slopes of the wave are equal in their degree of incurvation or steepness. This symmetrical form is restricted chiefly to the gentler or flatter undulations, and especially to those of considerable amplitude. We do occasionally meet with steep waves of the strata, having a nearly equal inclination on both their sides; but these are generally broken curves, exhibiting a snap or sudden angle at the anticlinal or synclinal axis, in place of the gradual arching, which is the normal form of all regular crust undulations.

Normal Flexures.

Another and more prevailing form displays a more rapid incurvation, or steepening of the flexure, on one side than on the other. Waves of this type have been called *Normal Flexures* by my brother and myself, in our descriptions of the Appalachian chain, where they are very common. They are to be seen abundantly in the Jura, and in the exterior hills of the Alps. They abound, too, in the undulated palæozoic region of Southern Belgium, and are a marked feature in the coal-basins of that country.*

These flexures prevail wherever the forces that disturbed the crust were neither excessively intense nor very feeble. They usually hold an intermediate position geographically, answering to the middle place they occupy as respects energy of undulation between the groups of flat symmetrical waves, and those which are closely folded, to the description of which I next proceed. Almost invariably, those of a simply undulated tract, exhibit their steeper slopes directed all to one quarter.

Folded Flexures.

This *third* and remaining class consists of flexures in which there is an inversion or doubling under of the steeper side of each convex curve or wave. When this structure is at a maximum, the folding back, downwards, of each convex or anticlinal arch amounts almost to a parallelism of the two branches or sides of these curves; and where there are several such foldings, alternately convex and concave, the strata may be said to be crimped or plicated into one dip, though the entire change of inclination through which the inverted portions have been bent, amounts to the supplement of the angle of the dip or the difference between the apparent dip and 180°. It is a necessary feature of all such folded flexures, that the approximately parallel sides of the folds dip obliquely and not perpendicularly to the horizon. They are, therefore, but exaggerated instances of the class of normal flexures, or those where one branch of the curve is steeper than the opposite. As in the case of the normal flexures, the more incurved sides of these folded waves all look the same way.

Axis Planes.

It is convenient, for the purpose of expressing the kind of flexure, its degree, and its direction, to make reference to the geometric planes which bisect or equally divide the anticlinal and synclinal bends. These imaginary planes we have called the axis planes of the undulations, being those which include all the horizontal lines or axes round which the individual concentric strata have bent in the act of undulating or folding. In the first-described class of flexures, or those of symmetrical curvature, each anticlinal and synclinal axis plane is necessarily perpendicular to the horizon. In the second class, or the normal flexures, these axis planes are necessarily not perpendicular, but steeply inclined to the horizon, and their deviation from the perpendicular is in proportion to the difference of inclination, or of incurvation of the two slopes of the wave, modified, according to a certain law of variation, with the dip. In the *third* class, or that of flexures with *inversion*, the axis planes are likewise not perpendicular; and it will be found that, in the great majority of instances, they dip with a less degree of steepness than the planes bisecting waves of the normal or other unsymmetrical type. Indeed, it may be stated generally, that, just in proportion as the flexure departs from the symmetrical wave form, through greater and greater inequality of dip, up to parallelism of the inverted with the uninverted branch of the curve, the axis plane departs from the perpendicular direction, to assume a less and less inclination to the horizon. In many districts of extreme plication of the strata, for instance in the Atlantic slope of the middle and southern States of North America, also in the Bernese Oberland, in the Ardennes, and in North Wales, the axis planes dip at an extremely low angle, consequent on the excessive amount of horizontal movement which the strata have undergone in the act of folding.

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* See DUMONT's Memoir sur les Terrains Ardennais et Rhenan, &c.

So nearly parallel are the inverted to the uninverted sides of the folds,—the axis planes all, of course, dipping one way,—in many districts of close plication, that the detection of the anticlinal and synclinal bends is not a little difficult, especially where the sections, natural or artificial, are not perfectly clear of superficial debris. In such cases the whole plicated mass looks as if it contained but one dip, or consisted of only one thick sequence of deposits, instead of am uch thinner formation many times reduplicated. To add to the liability of error, such bodies of folded strata are especially subject to that condition of jointage which is called *slaty cleavage*. In this structure, as I shall presently show, the divisional planes not only tend to obscure the original planes of sedimentation by their greater conspicuousness, but they often, by observing a very prevailing parallelism to the general dip of the folded beds, or, more strictly, to their axis planes, effectually disguise the anticlinal and synclinal curves. It is from these circumstances, and not from any erroneously supposed effect of truncation or denudation, actually to remove the anticlinal bends of the strata, that it is frequently so difficult to detect the true order of original superposition and the real thickness of closely plicated formations. Of course, no erosion upon an anticlinal axis, however closely folded it may be, can obliterate the bends in those beds which have their curves below the level reached by the denuding agent.

Crust Waves Straight and Curvilinear.

Regarding the great flexures of the crust as individual waves, which in truth they seem to be, we find them exhibiting, not only the above differences in the sloping of their two sides, but marked differences of form when viewed longitudinally. Thus, many are of extraordinary straightness; some of the larger simple anticlinals of the Appalachians being more than 100 miles in length, without any material or even perceptible horizontal crooking or deviation in their crest lines.

Others again are curved, some of these sweeping convexly towards the quarter of chief crust dislocation and metamorphism, others curving convexly from it, but we never find these two classes associated in the same group, and in the Appalachians, never even in the same segment of the undulated zone. In some districts of this and other chains, some of the principal curvilinear anticlinals and troughs are quite as extended in length as the great axes which are straight. They appear to be independent waves generated from curvilinear fractures of the crust, and not to be merely the bending terminations of adjacent rectilinear flexures. One of the most interesting features belonging to some of them is, their extent of curvature, and the graceful continuous smooth sweep which their curving axes present, often without jog or hitch, from one extremity to the other. This crescent-like form is developed in a high degree in those curving sections of the Appalachian chain, where the waves are convex north-westward, or from the quarter of maximum dislocation—the Atlantic slope. In the Juniata division of the chain in Pennsylvania, some of the curving anticlinals, 80 and 100 miles in length, change their trend between their two extremities as much as 40°; and in the Delaware division of the same chain, which also bends with a concave sweep to the north-west, the deflection in more than one great synclinal trough, and anticlinal axis, is not less than 60°. This fact of the curvilinear form of anticlinals and synclinals of great length in this was long ago offered by us, as a phenomenon incompatible with the generalization of the eminent French geologist M. Elie de Beaumont, who conceives that the lines of elevation of the crust have been great circles of the sphere, and that those of a given geological epoch have invariably observed one constant direction. The whole of the Appalachian chain having been demonstrably corrugated into its present undulations at one epoch, that of the end of the coal period, the simple fact that its different groups of waves deviate as widely in direction from each other as 60°, while those of each group are reciprocally parallel,—the whole chain indeed, if subdivided on this principle of direction, including not less than eleven conspicuous segments,—is itself enough to show that no particular constancy of relation can be established between the dates of elevations, and the mere directions of the lines or axes of the strata. But this other fact of so marked a change of direction in one and the same axis, as displayed by these crescent-shaped waves, is, if possible, in still more striking contradiction with that hypothesis. Another fact connected with the groups of curving waves in the Appalachian chain is particularly deserving of mention in this place, from the bearing it appears to have upon the question of the direction of the pulsatory or wave-like motion of the crust, at the time of the permanent production of the flexures. It is this—the individual waves in all the segments of the chain which are convex north-westward exhibit, as already said, a continuous symmetrical crescent-like curvature; those, on the contrary, which are included in the other curvilinear districts, convex to the south-east, or towards the region of dislocation and metamorphism, present a much less regular incurvation along their anticlinals and synclinals, and a far greater amount of interference and of dislocation. These appear indeed to be the sections of the chain, where the greatest amount of tangential wrenching, rupturing, and warping of the crust has taken place, and where the greatest amount of transverse hitching and fracturing has happened to all the strata. The causes of this difference will, I think, be seen, when I shall have developed our theory of the mechanical forces which undulated the Appalachian strata, and set in motion the stupendous billows of the crust, which resulted in the elevation of these mountains. An inspection of the best maps and sections of the more disturbed European zones, leads me to believe, that a similar contrast prevails between the curvilinear waves which are convex to the districts of disruption, whence I suppose them to have proceeded, and those which are concave to the same quarters; but before this law in all its generality can be established, geologists must institute far more critical researches into the physical structure of those undulated and plicated districts than they have hitherto conducted.

GRADATIONS IN FLEXURES.

Succession from the Folded to the Symmetrical Waves.

Several phenomena of gradation will be found to display themselves when we cross any broad belt of plicated and undulated strata. Starting from the side of maximum disturbance and contortion, invariably the quarter of maximum igneous action,—displayed either in Plutonic eruptions through the crust, in crust dislocations, or in metamorphism of the sedimentary rocks,—the flexures first met with are of the obliquely plicated form. Advancing towards the middle of the zone, the folds become obviously less close, and proceeding still farther, they gradually open out, displaying more conspicuously their anticlinal and synclinal curves, until the inverted side of each wave becomes only perpendicular. This perpendicular altitude of the steep side soon becomes a dip towards the opposite quarter from that previously observed by both sides, and as we proceed, the steepness of the slope of the wave now rectified in position, grows progressively less and less, until on the far side of the zone, both slopes approximate to equality.

Expansion of the Waves as they pass from the Folded to the Symmetrical Form.

Concurrently with this gradation, there is a progressive opening out of the spaces between the crests of the successive waves, such indeed as to amount in the Appalachians, and sundry other broad regions of crust-undulation, to an enlargement by many times of the amplitude of the more compressed class of flexures.

Progressive Flattening down of the Flexures.

A third feature of gradation shows itself in the progressive sinking or flattening down of the successive individual flexures, until these finally pass into horizontality. These three types of form in the waves, as respects their expansion, their increase of relative distance or amplitude, and their declining height, are conspicuously discernible, wherever we cross the great Appalachian chain of the United States, by any section, in a direction from south-east to north-west. An inspection of the engraved sections illustrating our paper on the physical structure of the Appalachians,* or an examination of the more numerous similar diagrams explanatory of the geological surveys of New Jersey, Pennsylvania, and Virginia, will amply avouch for the correctness of this generalization. It is further borne out in the published reports of the Government Survey of Canada, where the plicated structure of the green mountain range of Lower Canada, along all its south-eastern border, and the universality of the south-eastward dip of the folded strata, —in other words, of the dip of the axis planes,—is very distinctly set forth by Sir WILLIAM LOGAN.

Not only does the entire chain in its breadth exhibit a general gradation in the several features here described, but each of its great component groups of flexures, presents the same progressive opening, recession, and flattening down of its waves in the same uniform north-west direction.

Similar phenomena of gradation will, we feel assured, disclose themselves in any section made from the Taurus range, north-westward through the Rhenish Provinces and Belgium, where, on the one side, the more ancient and much metamorphosed strata at the base of the Palæozoic system, according to the observations of MURCHISON and SEDGWICK, present much reversal of the dip, and where one and the same dip, namely, to the south-south-east, is continued with very few exceptions across a belt of 50 miles; whereas, on the opposite or northern side of the zone, as is well shown in the beautiful sections of M. DUMONT, the flexures of the Belgian coal-fields are of the normal type, and much more open and dilated.

Nowhere perhaps in Europe are these gradations in the undulations of strata more beautifully exposed than on the flanks of the Alps. Deep in, towards its higher central igneous chains, the plication of its stratified rocks is excessive, and the inclination of the axis planes remarkably low; but advancing outwards, the waves gradually lift their crests, throw forward their inverted sides, and assume that type of flexure which we have called the normal one; while, at the outward base of the mountains just before these undulations are concealed by the overlapping tertiaries of the plains of Switzerland, and of Northern Italy respectively, the curves become, in many instances, broad, depressed, and almost symmetrical in form.

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* Transactions of the Association of American Geologists.

From the descriptions here given of the structure of the Appalachian chain and other disturbed districts, it is obviously a general law, that the axis planes of the flexures are not only inclined all in one prevailing direction, though at different angles, but that they dip invariably towards the quarter or zone of maximum disturbance and rupture of the crust.

Fractures or Faults in Tracts of Undulated Strata.

Two classes of dislocations abound in all zones of plicated and undulated strata, where the crust waves exhibit much steepness, and especially where they have the inverted or folded form. By far the most numerous, though the shortest and least conspicuous class, are the breaks or faults which run approximately transverse to the strike of the anticlinal and synclinal axes. These may be extensively recognized in the Appalachians, where they are a primary cause of the deep ravines, or breaches through the ridges, which furnish passage to nearly all the rivers, and even lesser streams which drain this chain. Such ravines are especially frequent near the extremities of the large anticlinal waves, particularly where they have been cut through along their crests by denuding waters, and have given rise to valleys of elevation and erosion, inclosed by monoclinal, outward-dipping, sandstone ridges. It would seem as if the elliptical folding round of the strata towards the ends of the great denuded waves had caused the horizontal wrenching which resulted in these fractures. Mr William Hopkins, of Cambridge, has, in an able paper on the subject of dislocations affecting dome-shaped elevations of the earth's crust, indicated the true source, I conceive, of the double system of fractures to be met with in all elliptical anticlinal belts. An elongated anticlinal wave is, in truth, only a greatly lengthened elliptical dome, in which the radial cracks caused by a maximum tension in the strata transmitted from the more central portion of the crust-wave, are distributed, some of them longitudinally, others transversely, as respects the anticlinal axis, the transverse ones multiplying themselves where the elliptical strain has been greatest, towards the two extremities of the waves.

The other far more conspicuous class of dislocations connected with these crust undulations, are the great longitudinal ones. These are of frequent occurrence in the more contorted portions of the Appala-

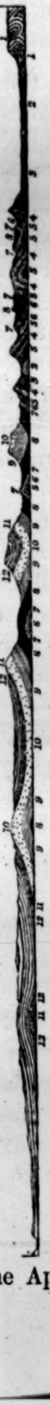


Fig. 1.

Generalized Section of the Appalachian Chain from north-west to south-east, through the Juniata district of Pennsylvania.

chian zone, especially in those where the chain is convex to the south-east, and in the straight sections of South-western Virginia and Eastern Tennessee. But I am persuaded, from the descriptions of geologists, and from my own observations that the fractures of this class are equally numerous in the Jura Mountains, in the Alps, in the district of the Ardennes, in Belgium, and in the mountain chains of Scotland. A leading feature of these great fractures is their parallelism to the main anticlinal axes, or lines of folding of the chains to which they belong. They are, in fact, only flexures of the more compressed type, which have snapped and given way in the act of curving, or during the pulsation of the crust. They coincide, in the great majority of instances, neither with the anticlinal nor the synclinal axis planes of the waves or folds, but with the steep or inverted sides of the flexures, and almost never occur on their gentler slopes. This curious and instructive fact may be well seen in the Appalachians of Pennsylvania and Virginia, by tracing longitudinally any one of their great faults from its origin on the steep flank of an anticlinal wave along the base of its broken crest to where the anticlinal form is resumed again. The following brief description, from our memoir on the Physical Structure of the Appalachians, taken from the Transactions of the American Association, will show the general phases through which these fractures pass:—

"From a rapidly steepening north-west dip, the north-western branch of the arch (or flank of the wave) passes through the vertical position to an inverted or south-eastern dip, and at this stage of the folding the fault generally commences.

"It begins with the disappearance of one of the groups of softer strata lying immediately to the north-west of the more massive beds, which form the irregular summit of the anticlinal belt or ridge. The dislocation increases as we follow it longitudinally, group after group of these overlying rocks disappearing from the surface, until, in many of the more prolonged faults, the lower limestone formation (Cambrian or Lower Silurian) is brought for a great distance, with a moderate south-easterly dip, directly upon the Carboniferous formations. In these stupendous fractures, of which several instances occur in South-western Virginia, the thickness of the strata ingulfed cannot be less, in some cases, than 7000 or 8000 feet."

One of these enormous faults in South-western Virginia has a length of more than 80 miles, and is almost perfectly straight. It follows the south-eastern slope of Brushy Mountain, from the head of Catawba Creek to the vicinity of the court-house of Smyth county, engulfing all the strata of the south-eastern half of a synclinal basin, of which the Brushy Mountain remains as the other half. Where the dislocation attains its maximum intensity, or shows the greatest displacement of the strata, the lower formation,—the Auroral Appalachian limestone, the equivalent of the Festiniog group of England,—of one side of the fissure, rests in an inverted attitude, with a gentle south-east dip, directly on the south-east dipping Vespertine grits and shales—represented in Great Britain by the lowest Carboniferous strata,—forming the other wall of the fault.

General Parallelism of the Faults to the Axis Planes.

It is a very general feature of the great longitudinal faults, whether these coincide with the anticlinal and synclinal axis planes, or occur, as they more frequently do, on the steep sides of the flexures, to dip in the same direction with the axis planes. In the Appalachian chain their inclination, therefore, is almost invariably towards the south-east. A consideration of the nature of the forces which have folded and ruptured the strata, shows that such a direction of their dip is an almost inevitable consequence of the undulatory movement. It is only in districts of low symmetrical crust undulations, or those where the strata are absolutely flat, that the great fractures descend perpendicularly.

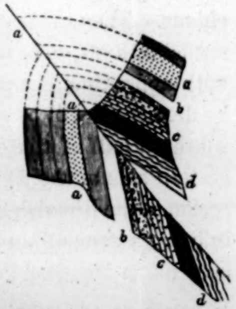
In corrugated zones, like those of the Appalachians, the Alps, and the Ardennes, the magnitude of these main longitudinal fractures, both as respects length and vertical displacement of the dislocated strata, is in proportion to the sharp bending or close folding of the waves to which they belong. Thus they invariably possess their grandest dimensions, in the south-eastern or most plicated belt of the Appalachians, or on that side of the zone where the crust movements have been most energetic.

Uninverted side of Wave usually shoved over the Inverted.

This obliquity or dip towards the quarter whence the movement has proceeded, is evidently the cause of that overlapping of the newer, less-lifted side of the wave in which the fault lies, upon the steeper, more perpendicular, or inverted flank; for the forward or horizontal thrust which accompanied the propagated wave-movement resulting in the fracture, has, when this once occurred, found an inclined plane, up which the uninverted slope of the wave slid over the edges of the strata composing the inverted side. In many instances, as the Appalachian sections will prove, one flank of the wave has been shoved forward and upward, unconformably, upon the crushed and buried flank to an enormous distance. Subsequent erosion having cut down the higher strata of the updriven, gently-sloping side of the wave, its lower beds are exposed to view, in immediate contact with the unremoved upper strata of the other side. Where the lower formations, cut into by the water on both sides of the fault, have been equally easy of excavation, especially when they are all of identical composition, as in the case of the great lower Appalachian limestones, the denuding waters have so effectually planed down the great inequalities of surface at first caused by the dislocation, as sometimes to have left in the landscape almost no external traces of the gigantic rupture which lies beneath the soil. It is, then, only by a recognition of the ages of the respective strata, thus abruptly placed in contact, and usually, though not always, by some sudden difference of dip, that we are enabled to detect the presence and the magnitude of the dislocation.

It seems also necessary, on this occasion, to explain the effects of those great longitudinal obliquely-dipping faults, when they occur directly in the anticlinal and synclinal axis planes, which are their occasional positions. The same forward upward-sliding, just described as having occurred where the fracture is between the anticlinal and synclinal curves, must have taken place where it has coincided with these, and as the movement must necessarily have been in the same direction, lifting, that is to say, the lower strata cut by the fault, upon the edges of higher and higher beds, in the forward propulsion of the flat side of the broken wave, we have no difficulty in understanding how fractures in these positions, as well as in the other already spoken of, must have given rise to that very common phenomenon of the dipping of newer formations under older ones in plicated and dislocated countries, like the Alps and Appalachians. This puzzling feature of stratification, long an enigma to geologists, can, I conceive, be explained upon no other analysis than that which is here given, namely, the oblique folding of undulated strata,—the obliquity of the planes of the faults, either coincident with, or parallel to, obliquely dipping axis planes,—and the forward upward thrust of the uninverted upon the inverted broken strata, through a tremendous tangential force incident to a wave motion.

Fig. 2.



Section of a longitudinal dissection showing the plane of fracture and the uninverted strata driven forward upon the inverted.

EXEMPLIFICATION OF THE LAWS OF FLEXURE, BY THE PHENOMENA OF SOME OF THE UNDULATED ZONES OF EUROPE.

Belgium and the Rhenish Provinces.—Embracing in one view the undulated districts of Southern Belgium, the Rhenish Provinces, the Westphalian coal-field, the Ardennes, the Hundsruck, Taurus, and Hartz ranges, as described and mapped by M. DUMONT and other geologists, we can discern most distinctly all the phenomena of flexure and of dislocation of the strata, here indicated as characteristic of the structure of the Appalachians. We there perceive a wide zone of crust undulations having its strata most invaded by igneous rocks, and most ruptured and metamorphosed, along its south-eastern side, and displaying its most ancient sedimentary formations in a state of close plication, with innumerable inversions of the dip, imparting to wide tracts one uniform parallel inclination towards the south-east. Crossing the zone north-westward, we enter newer and newer strata, until we come to the undulated coal-field of Westphalia or Belgium, our traverse taking us from the non-fossiliferous formations, at the very base of the Palæozoic system. In whatever meridian we make our section, we find the north-west sides of the waves, with few exceptions, steeper than the south-east ones, not only where they are inverted, but where they have a normal dip. We find, moreover, as we advance, that the waves grow more and more open, and that the distances between them increase, that they subside in height, and that the two slopes approximate nearer to equality. These gradations are admirably disclosed in any traverse across the strike north-westward, from the water-shed of the Ardennes to the Belgian coal-fields of the Meuse. I can detect, in the features of Dumont's exquisite map of Belgium and the neighbouring countries, the very same relations of the longitudinal faults to the flexures which have engendered them, as those above described in the fractures of the Appalachians. They evidently occur, for the most part, on the north-west sides of the anticlinal axes, and cause older strata to ride upon newer ones plunging under them, with approximately parallel dip. Even the phenomena of cleavage, presently to be described, will be seen to exhibit the very same laws in the more metamorphic southern half of this wide zone of plication, which they present along the south-eastern side of the Appalachian chain, and the Atlantic slope bordering it. This region of the Rhenish Provinces and Belgium further agrees with the Appalachians, in being a zone of undulations and plications, where the folding movement has been all in one direction.

The Jura Chain of Switzerland.—The Jura chain of Switzerland, as I pointed out in 1848, in communications to the Geological Society of London, and in 1849 to the American Scientific Association, is another very interesting belt of crust waves, displaying, in its structure, a close resemblance to the Appalachians.

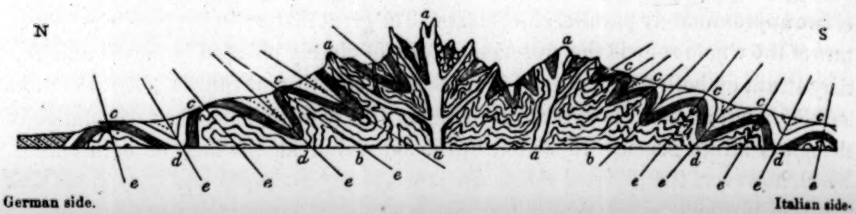
It embraces, like the American mountains, many groups of waves differing in the directions of their axes in different districts of the chain, but the individual groups composed of waves which are remarkably parallel. Few of these undulations exhibit actual inversion of their steeper sides, the dip only in some instances passing the perpendicular, and generally not exceeding on an average 70°, the gentler or opposite slopes having a mean slant of about 40°. In four traverses which I made across this chain, I observed one almost invariable law as to the direction of the steep and gentle sides of the undulations, or, in other words, of the axis planes. Contrary to first anticipation, and to the belief of many Swiss geologists, I found the steeper curvature of the waves directed toward the Alps, and not from them, implying that the crust movement which lifted these grand and picturesque arches proceeded from the north-west, and not from the chain of the Alps. This also is a belt whose undulations are chiefly in one direction.

The Alps.—The great chain of the Alps is much more complex in its structure than either of the undulated zones yet described. It contains but few waves of the open or normal type, but innumerable close foldings or plications. Throughout a great portion of its length, this lofty and rugged zone of mountains consists of two approximately parallel chief crests. The great feature in the geological structure of the whole zone is the presence of belts of closely plicated Mesozoic and Tertiary strata on both flanks of each of these great constituent ranges. But the most striking, and, at first view, perfectly enigmatical feature, is the inward plunge of the newer strata beneath the older, in the sides and at the base of both chains. When, however, the plicated strata are structurally arranged and traced, we find that this phenomenon assumes the character of a symmetrical folding of the rocks in two opposite directions from each high central axis. The individual foldings, with scarce an exception, lean outwards from the central tracts of the mountains, or from the quarters of igneous disturbance, rupture, and maximum metamorphism of the crust. In other words, the axis planes of the plicated strata of the flanks of the Alps dip inwards towards the centre of the chain; those nearest to it at a low angle, and those more remote at angles steeper and steeper as the waves recede, expanding to the outer base of the range. High on the flanks of the Alps, or, what is the same thing, deep in towards the roots of the mountain, where only the synclinal bends, of the flexed strata, have been protected from denudation by inward folding, these closely compressed troughs lie pinched in between the older strata in oblique inward inclination. The transverse sections expose these bendings, which are called Vs by some of the Swiss geologists. Here then we behold an exact counterpart in the stratification or structure of a single flank of the Alps, of that folding with inversion which characterizes the Appalachian chain, or that of the Ardennes, a single side of the Alps being the equivalent of the whole of either of those zones; it consists, that is to say, of a belt *undulated in one direction.* Crossing the Alps, or rather one of its component great chains, we find another *similar* belt of the same strata, plicated in the same way, with their axis planes dipping also under the crest or orographic axis of the mountain, but of course, to the opposite quarter of the compass as compared with the plicated zone of the other flank. This is, I conceive, a true picture of that feature which, hitherto imperfectly analysed, has been called by some of the geologists of Switzerland, expressively enough,

The Fan-like Structure of the Alps.

Viewed as a single chain, this mountain system consists, then, of two belts undulating in opposite directions; but, as already stated, it is for the most part of its length a double chain; and I think each range, especially, where these are widest apart has a plicated belt of strata upon each of its slopes, so that, for some districts at least, the fan-like structure is twice repeated; in other words, there are four belts of closely folded waves, each having its axis planes dipping towards the base of its own high mountain system.

Fig. 3.



Generalized Section of the Alps, displaying the dipping of the folds of the strata on both sides in towards the igneous axis.

a, Igneous rocks in central axis of the chain. *b*, Gneissic and other older strata.
c, Anticlinal flexures. *d*, Synclinal flexures, or Vs. *e*, Anticlinal and synclinal axis planes.

A conspicuous and pervading cleavage structure coincident in the direction of its dip, as I shall presently show, with the oblique axis planes of the folded rocks, contributes greatly, I conceive, to the illusive phenomenon of an inward dip of all the strata, or to that general feature which has been called fan-shaped.*

This inward dip is rendered still more obvious by the circumstance, that the foliation or crystalline lamination of the more altered strata, itself obeys very generally a similar law of parallelism to the axis planes of the flexures. Where this crystalline grain of the rocks does not coincide with the stratification, it exhibits a great tendency to a coincidence of dip with any system of cleavage planes belonging to the same or other parts of the mass. In either case, it will dip inwards towards the igneous axis of the chain, if the strata possessing it are themselves closely folded in conformity with the prevailing law. But the phenomena of cleavage and foliation will be noticed afterwards. We now proceed to discuss

GENERAL PHENOMENA OF SLATY CLEAVAGE IN THE APPALACHIANS AND OTHER ZONES OF PLICATED STRATA.

Cleavage parallel with, but independent of the Dip of the Strata.

It is now a good many years since Professor SEDGWICK and other geologists announced the important general fact, that the structure called *cleavage* pervades the altered strata affected by it, in directions independent of their bedding or laminæ of deposition. That eminent geologist further announced that these planes are approximately parallel to each other over large spaces of country, however contorted the dip of the rocks. He likewise enunciated a second general law of much importance, "That when the cleavage is well developed in a thick mass of slate rock, the strike of the cleavage is nearly coincident with the strike of the beds." Subsequently Professor PHILIPS gave to this rule of the cleavage a still more comprehensive and exact expression, when he stated to the British Association in 1843, that the cleavage planes of the slate rocks of North Wales were always parallel to the main direction of the great anticlinal axes. Other geologists have abundantly confirmed these generalizations. Since 1837, these phenomena of the close parallelism of the cleavage planes of a given district with each other, and with the main axis of elevation of the district, have been constantly observed and recorded by my brother Professor W. B. ROGERS and myself, in our Geological Surveys of Virginia, Pennsylvania, and New Jersey.*

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* From the analysis above given of the structure of the sides of the Alps, it will be seen, that I entirely concur with Professor JAMES FORBES, and with all the more eminent of the Swiss geologists, in recognizing the fan-like dip of the newer strata, Tertiary and Mesozoic, conformably in appearance at least under the older strata, metamorphic and gneissic, of the higher more central tracts, and that I dissent entirely from the theoretical section offered by Mr DANIEL SHARPE.

In 1849, I submitted to the American Association for the Advancement of Science, at the annual meeting held at Cambridge, Massachusetts, in a communication on the analogy of the ribbon structure of glaciers to the slaty cleavage of rocks, a statement of what I had for some years past regarded as the true law of the direction and position of the cleavage planes of a district of undulated and plicated strata.

In its simplest expression the rule is, that *the cleavage dip is parallel to the average dip of the anticlinal and synclinal axis planes*, or those bisecting the flexures. The generality of this rule was shown on the occasion mentioned, by sections exhibiting the flexures and cleavage in the Appalachians, in the Alps, and in the Rhenish Provinces; and I have since become convinced of its universality from the inspection of the phenomena of other districts, and from a study of the descriptions and sections of geologists. Want of time at present prohibits me from citing the abundant evidence for this law to be found in the best recently printed memoirs upon slaty cleavage; but I hope to be able ere long to give my own observations in support of the highest British geological authorities, who, unaware of the relationship itself, have furnished the most satisfactory data for the recognition of it. I cannot, however, refrain, in this place, from sustaining the generalization I am here venturing to put forth, by instancing the support it receives from the excellent descriptions recently given by Professors HARKNESS and BLYTH of the Cleavage of the Devonians of the South-west of Ireland. In their paper in the Edinburgh New Philosophical Journal for October 1855, they not only establish an agreement between the strike of the cleavage planes with that of the several rolls (or anticlinals) which affect the island of Valentia, but they show that while the cleavage dip is southerly, the anticlinal "curves have been pushed over in a more or less northerly direction," inverting the carboniferous limestones and coal measures. Their general statement is, that the cleavage structure of rocks does not result from the simple rolling of the strata, but from this cause combined with a considerable amount of pressure; and this latter force acting from the south, has pressed over the strata in a series of oblique curves to the north, and given to the inclined cleavage its more or less of a southern dip. They support the doctrine of Mr SHARPE respecting the cleavage of rocks,—"That there has been a compression in the mass in a direction everywhere perpendicular to the planes of cleavage, and an expansion of this mass along the planes of cleavage in the direction of a line at right angles to the line of incidence of the planes of bedding and cleavage," or, in other words, to the direction of the dip of the cleavage. From this view of the mechanical nature and direction of the force engendering cleavage, I beg leave respectfully but explicitly to dissent.

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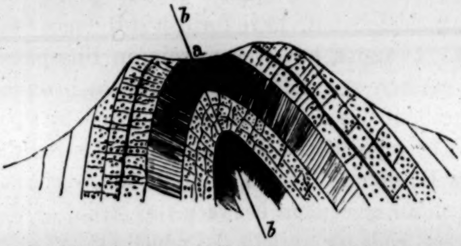
* See Ann. Reports on those Surveys, 1837–40, and other Essays.

Fan-like Arrangement of the Cleavage at the Anticlinal and Synclinal Axis Planes.

A second general fact or law of direction of the cleavage planes in folded strata must be here enunciated. At first view it is in seeming contradiction with the universality of the primary rule above stated, of the invariable approximate parallelism of the cleavage planes to the axis planes of the flexures; but closely examined, it will be seen, I think, to be in beautiful accordance with that law and with my hypothesis of the origin of the cleavage structure. The rule is this, that where the cleavage is fully developed, and the anticlinal and synclinal flexures are also conspicuous and very sharp, the cleavage planes immediately adjoining those bendings are not parallel to the axis planes, but partially *radiate from them in a fan-like arrangement* upward in the anticlinals, and downward in the synclinals.

This aberration from the normal direction is furthermore different in degree upon the two sides of the geometric axis plane, being usually greatest upon the inverted or steep side of the wave.

Fig. 4.



Fan-like Arrangement of Cleavage at an Anticlinal Axis.
a, Cleavage in the Shale. *b*, *b*, Axis Plane.

Another aberration of the cleavage planes from their normal direction of parallelism to the axis planes, is their tendency to conform partially to the dip of the strata, when the two are nearly coincident. This operates to flatten the inclination of the cleavage upon the gentler slope of each wave, and steepen it upon the more inclined one; and as in every belt of uniform flexures closely plicated with inversions, the uninverted or normal dips greatly exceed the inverted ones, it produces in such cases a prevailingly lower inclination in the planes of cleavage than in the planes bisecting the flexures.

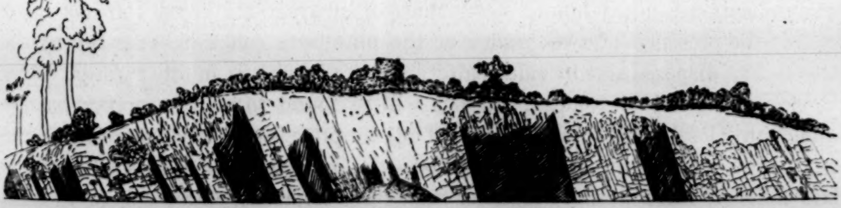
Relation of Cleavage to the Mechanical Constitution of the Strata.

There is yet another law respecting cleavage; it is the dependence of this structure upon the mechanical texture, and possibly upon the chemical composition, of the fissured rocks.

Geologists have for several years recognized the fact, that in formations composed of alternations of the coarser mechanical rocks, such as silicious grits and conglomerates, with fine-grained argillaceous beds, as slates, shales, or marls, the coarse beds are unaffected by cleavage, while the fine-grained ones are often pervaded by it. Indeed, one may observe in a given locality almost a strict proportion between the degree of intimate fissuring of the rocks by cleavage planes, and the degree of comminution of their particles.

Connected probably with this interruption in the distribution of the cleavage-condition through such heterogeneous groups of strata, I have observed another general fact of modification of the cleavage planes, which should not be passed unnoticed here. They tend in the fine grained argillaceous beds, to curve a little from the normal direction into an approach to parallelism with the surfaces of bedding of the adjoining coarser mechanical deposits, presenting, in a transverse section, a kind of gentle sigmoid or double flexure. This is well shown in the cleavage-traversed rocks at the base of the anthracite coal formation of Pennsylvania, especially in the transition or passage beds which connect the Umbral red shales of that region, with the base of the coal-sustaining conglomerate, and also where these shales alternate with the upper coarser members of the Vespertine sandstone. The small section here appended, showing the cleavage in one of these groups of alternation of red shale and sandstone, from a railway cut near Ashland, in the middle anthracite coal-field, exemplifies well the phenomenon referred to.

Fig. 5.



Beds of Red Shale with Cleavage alternating with beds of Sandstone without Cleavage; Cleavage curving towards parallelism with the bedding at its boundaries. Section near Ashland, Pennsylvania.

The tendency, here shown, in the cleavage planes to conform to the planes of bedding, where abrupt changes of composition interrupt the continuity of the fissures, is but another variety of the phenomenon already adverted to, of a deflexion of the cleavage in bands of plicated strata towards a parallelism with the gently dipping slopes of the anticlinal waves. This remarkable fact of an intimate dependence of the cleavage upon the composition and mechanical texture of the structure is, I conceive, of itself sufficient to refute the hypothesis somewhat in favour at present, of the purely mechanical origin and nature of the cleavage-producing force; for we cannot conceive how a mechanical force, either of compression or of tension, transmitted, as necessarily it must be, very equally through parallel layers of coarse and fine materials, should have exerted no fissuring action the moment it reached the surfaces of the coarser beds, and yet have been able to cleave into thin parallel slaty laminæ the whole body of the fine-grained argillaceous strata. One would more naturally suppose that the less firmly aggregated softer mud rocks or shales would have been even less easily fissured by sharp cleavage joints, than the more massive and better cemented grits. It is of importance to notice here, that subsequent disruption of the strata may change the normal position or dip of the cleavage, after its formation, and give rise to some of the apparent deviations from the general law of direction above enunciated.

The Cleavage Susceptibility alternately greater and less in Parallel Planes.

Cleavage is a susceptibility in rocks of a certain composition, and in a particular stage of metamorphism, to split in definite straight parallel planes. The cohesive force is obviously at a minimum of intensity in the direction perpendicular to these planes. In the other two rectilinear axes of the cube, one side of which is coincident with the cleavage plane, the force of cohesion next in degree of intensity is the horizontal one, or that in the direction of the strike of the cleavage, while the most intense cohesion of all is that in the direction of the cleavage dip. It is in this latter direction that the molecular forces of attraction engendering incipient crystallization seem to have been most powerfully awakened while the polarities have been feeblest in the lines perpendicular to the cleavage planes; but apart from these three directions and grades of corpuscular force, we have indications, in any homogeneous mass of cleavage-traversed slate, or other rock, of the presence of two grades of the minimum cohesion, constituting the cleavability, disposed side by side in alternate parallel order; in other words, where the cleavage is fully developed, the rock will be found to contain certain nearly equidistant closely contiguous planes of maximum cleavability, or, what is the same thing, of minimum lateral cohesion—the material of each thin plate of the slate cohering more strongly together than these adjacent plates cohere to each other. The existence of such planes is indicated by the manner in which any mass of very cleavable slate, long exposed to atmospheric agencies, invariably breaks up; as we may see in any naked outcrop. If the cohesion of the mass in a direction perpendicular to the cleavage planes were equally strong in all parallel planes that we can imagine pervading it, it is impossible to understand how any uniformly acting disintegrating forces,—either expansion and contraction by heat, soakage and drying, or freezing and thawing,—could subdivide it by planes or fissures, so regularly distributed as we find them. These could only have arisen, I conceive, from the presence of parallel planes of weaker and stronger cohesion. In this interesting structure, we discern a striking analogy to that alternation of thin plates of solid blue crystal ice, and white porous ice of less cohesion, which is so distinct a feature in the fully developed ice of glaciers, and which has been expressively named by Professor JAMES D. FORBES, the ribbon structure.*

FOLIATION.

The relations of the foliation or crystalline lamination of metamorphic strata to the cleavage planes, and the planes of stratification, come next to be considered. Two facts may be stated of foliation, which possess, perhaps, the constancy of general laws. One of them is, that this structure, as it is seen in gneiss and mica schist, observes, when the strata are not traversed by cleavage, an approximate parallelism to the original bedding. Apparent exceptions to this rule occur in several localities near Philadelphia, and elsewhere in the United States, and have often been noticed in Europe, by Mr D. SHARPE, and other good observers; but all of them can be reconciled to the general fact, and reduced, it is conceived, to one comprehensive law, namely, that the planes of foliation, or the laminæ, formed by the crystalline constituents of the foliated rocks, are *parallel to the planes or waves of heat* which have been transmitted through the strata. Wherever large tracts of the gneissic rocks retain a nearly horizontal undisturbed position, the foliation is almost invariably coincident with the stratification, and in this case, the wave of heat producing the crystalline structure can only have flowed upwards through the crust, invading stratum after stratum, in parallel horizontal planes. Again, when injections of granite occur in uplifted gneissic strata, the crystalline lamination is generally seen to be parallel to the plane of outflowing temperature.

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* In a communication submitted to the American Association for the Advancement of Science in 1849, I attempted to show this analogy of the ribbon structure of glaciers to the slaty cleavage of rocks, in the following remarks:—"The ice of glaciers consists of thin alternate parallel bands or plates of blue crystal ice, and white porous ice, each not more than one-third or one half of an inch in thickness. These pervade the whole mass of every glacier, and are clearly exposed on the sides of the transverse fissures. Near the sides of the glacier, they are almost absolutely parallel with its mountain walls, but they sweep away towards its medial line, and form, like all the other planes which divide the glacier, a series of innumerable loop-like curves. This looped or festooned form is obviously caused in part by the downward tendency of the movement or flow of the semiplastic ice, and in part by the influence of the terminal moraine to induce that parallelism to itself, which the rocky sides of the glacier produce in the ice near them. The most general fact noticeable in relation to these structural planes, is the approximate parallelism to the rocky walls and terminal moraine confining the icy mass; or in other words, to the surfaces of higher temperature, which inclose the glaciers. However the direction of the ribbon lines may alter by irregularities in the onward flow of the glacier, their position near the region of the nevé is strictly parallel with the surface of the warmer mountain sides."

The other general rule is, that *the foliation is parallel or approximately so to the cleavage,* wherever these two structures occur in the same mass of rocks. This fact, recorded by DARWIN, of the gneissic rocks and clay slates of South America, has been noticed likewise by Mr D. SHARPE, Mr DAVID FORBES, Mr SORBY, and other geologists in Great Britain, and by the author, in many localities in Southern Pennsylvania, and in other districts of the Atlantic Slope. An interesting instance of such parallelism of the foliation to the cleavage, tending to show convincingly, that both phenomena are the consequences of one species of force, or only different degrees of development of the same molecular or crystallizing agency, is presented in the great synclinal trough of the lower Appalachian limestone, north of Philadelphia. On the north side of this trough, the Primal and Auroral rocks dip southward over a wide outcrop at a very regular angle of about 45°. On the south side they have been lifted into, and even a little beyond, the perpendicular position, so that the synclinal axis plane of the belt dips at an angle of 65° or 70° to the south. Neither formation shows cleavage structure on the northern side of the valley, the limestone being there of an earthy texture, and in thick massive beds, but on the south or upturned side, this limestone is altered into a mottled blue and white crystalline marble, and is pervaded with cleavage planes, dipping at angles of 70° and 80° southward. Many parts of the rock are like a foliated calcareous gneiss, thin laminæ of mica and talc dividing the slate-like plates of the marble. It is especially worthy of notice that the foliation of these mica and talc, composing some of the thin partings between the original beds of the limestone, is itself very generally parallel to the cleavage in the adjoining calcareous rock. Indeed, wherever the cleavage is excessive, the mass becomes, by introduction of fully developed talc and mica between its laminæ, a true foliated stratum. An especial interest annexes to cases of this kind, from their showing, that in the two contrasted conditions of the absence and presence of metamorphism in the two opposite outcrops of the same synclinal fold, both effects, cleavage and foliation, have originated at the same time, and from one and the same cause, and are, in truth, but different stages of the same crystalline condition, superinduced in the mass by high temperature, at the period of its elevation. The above general fact of the prevailing parallelism of the foliation to the cleavage, is but a corollary of the more general relationship already expressed of the parallelism of the resulting planes of crystallization to the waves of heat, which have produced the metamorphism.

EXAMINATION OF THE PREVAILING THEORIES OF ELEVATION.

Perhaps the most current notion respecting the force which has displaced and elevated the originally horizontal strata of the globe, is that which represents the granitic and volcanic rocks as forcibly injected in a melted state into fissures, and violently thrust in solid wedge-shaped masses upwards through the incumbent crust. That this is the prevailing idea, is apparent from the manner in which nearly all geological sections, even the most modern ones, designed to represent the relations of the Plutonic to the sedimentary rocks, are to this day constructed. Where igneous rocks constitute the whole, or a large portion of the central axis of a mountain chain, or even that of a simple anticlinal ridge, they are usually represented in cross sections, in the form of a broad wedge, and the stratified rocks are drawn as leaning upon the sloping flanks of the wedge or prism. This is not, I think, the true relation in Nature of the igneous to the sedimentary masses, as I propose to show from the following considerations.

Hypothesis of Wedge-like Intrusion of Melted Matter.

The notion of an upward wedging, or intrusion of molten mineral matter into or through the superincumbent strata, in the manner of a wedge, implies a function in the soft material which belongs to the mechanical action of a solid, and is incompatible with the dynamic properties of fluids. Until a fissure from below first penetrates or traverses the invaded overlying strata, it is not possible to conceive, that the liquid matter could introduce itself in the mode of a wedge. Some force must first crack the crust, and then the molten matter, flowing into the fissure may act as a narrow wedge or key, to keep the walls of the chink distended, but such plates of solidified refrigerated volcanic matter, known as veins and dykes, must necessarily be narrow, and have the shape rather of walls with parallel surfaces, than great wedges broad at the base.* They will also abound chiefly in the districts of subsidence, or in the concave waves, not in those of elevation, or in the convex, where the wedge-like form tapering upwards, is usually represented. Where a rupturing of the strata has taken place in a tract of elevation, or at an anticlinal, the fissure or fissures will be found to gape upwards, and the melted volcanic matter which has flowed to the surface, will be seen widening outwards and tapering as it descends, the very opposite of the form usually assigned to such outbursts, in the igneous axes of uplifted chains. So common is this upward enlargement of the Plutonic masses in certain regions, that it constitutes, I conceive, one element of the fan-shape or inward dip of the boundary walls of the rocks, so frequently encountered in the Alps and other much disturbed mountain systems. A true conception of the formation of a mineral vein or dyke will represent it as the *consequence*, not the cause, of the fissure which it fills, the real process being, not a protrusion of the fluid matter through the crust, breaking it in its passage as a solid wedge might, but an actual injection or pumping of it into the newly opened vacuous cavity, from the pressure or tension below.

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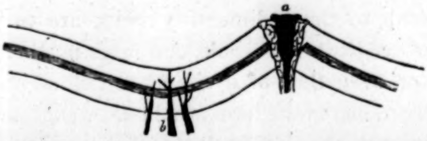
* It is in consequence of this natural expansion of surface-cracks outwards in anticlinals, that the miner so frequently finds his mineral lodes contracting or dying out as he descends. Several striking instances of this thinning of veins downwards could be cited, from the mines of the United States, situated in anticlinal flexures.

Fig. 6.



Dykes expanding upwards in Anticlinals and downwards in Synclinals.

Intrusion of the Igneous Rocks in Solid Wedges.

The other notion, frequently connected with the above idea, of a forcible propulsion of igneous matter through the crust, is that of the violent thrusting upward of volcanic or granitic matter already solidified, in broad wedge-like masses through the strata. This conception I hold to be at variance both with sound mechanical laws, and with the physical facts. For the solid igneous mass to have acted in the manner of a wedge, it is absolutely necessary that it should have moved freely upward through the opening in the strata, which it is supposed to have wedged apart and to have uplifted, and even corrugated, by lateral compression. But it is impossible to imagine such a slipping of the assumed granitic wedge past the edges of the strata confining it, since we can imagine no force acting downward upon these latter, to prevent their moving upward along with the wedge of granite, nor any localization of the force below, to prevent it operating on both alike. We have furthermore no evidence of that discontinuity between the igneous rock and the ruptured strata, which the notion of a sliding wedge obviously presupposes; but, on the contrary, every proof from general theory and from observed facts, that the two descriptions of rock are intimately bound together in closest crystalline contact, keyed together by veins, branching from the mass of the one into the fissures of the other, and even fused together by an actual incorporation of substance. Any upward movement, therefore, of Plutonic masses, bearing sedimentary rocks upon their flanks, cannot have been in the manner of a mechanical wedge; and those results—corrugation for example—of the adjacent strata habitually attributed by many geologists to an imagined wedge-like lateral thrust, must be accounted for upon some other sounder mechanical theory.

A modified form of this conception of an igneous wedge lifting and displacing the strata, assumes no sliding or wedge-like protrusion of the solid granitic matter past the edges of the rupture in the bedded rocks, but recognizing the inseparable cohesion of the two, regards the stratified masses flanking the anticlinal mountain, as merely borne upward by the uprising of the central igneous nucleus. I deem this notion to be a much truer picture of the procedure of nature; for it so far accords with what we notice in anticlinal districts having igneous crests or centres, that it represents the stratified rocks leaning against the walls of the great granitic central dykes, at steeper and steeper angles, the higher we ascend towards the summits. It is inexact, however, in picturing the granitic nucleus of the anticlinal mountain, as a wedge or broad prism tapering upward, for reasons already shown. Undoubtedly such a mountain, if we can imagine it denuded or truncated to lower and lower levels, would disclose a progressively increasing quantity of intrusive igneous rock, but this would be in the multiplication of the lateral granitic injections, and it is only in this erroneous sense, that the igneous nucleus can be regarded as a prism. Its cross section is branching rather than wedge-like.

The Upward Movement of an Igneous Dyke would tend to Stretch and not to Corrugate the Flexible Strata.

The view here admitted of the elevation of the igneous nucleus of a mountain, along with the strata which mantle it, while it is perfectly compatible with the hypothesis, to be hereafter advanced, of the origin of anticlinals generally, is wholly inconsistent with the somewhat current notion of the mode of origin of undulations and plications in the stratified rocks, by pressure from the tangential horizontal thrust of such uprising igneous axes; so far from its producing a lateral corrugating pressure upon the strata adjoining, and resting against it, a central granitic or other igneous dyke lifted vertically by one or many successive movements, paroxysmal or gradual, would rather *stretch* or distend the strata as it carried them upward than compress them.

Theory of Upward Tension against Lines or Points of the Crust.

Another common theory of crust movement and elevation of anticlinal belts supposes, vaguely, an upward tension or stretching of the crust of the earth along one or several lines, or at one or several focal points, without attempting to account for the linear or focal force, or to assign a cause for the restricted limits within which it is assumed to act. This conception, though confessedly indistinct, is frequently appealed to in explanation of the lifting of mountains, the corrugation of strata, and even the formation of regular groups of parallel anticlinal waves. I propose to consider its weak points.

Any theory henceforth admissible into physical geology, must explain the now clearly established general fact of the regular wave structure of the earth's disturbed zones. But this wave structure cannot be interpreted on the mere supposition of simply an upward pressure exerted either along one or many lines. The peculiar configuration of the crust waves, shown in this paper to be characteristic of them in all undulated regions, requires an hypothesis which will furnish both an undulating and a horizontal tangential motion; moreover, the ordinary doctrine, if it assumes the pressure from beneath to be exerted along a single line at a time, fails altogether to show us how this pressure could have shifted to new and parallel lines, or how it could take up new positions, and exhibit that relation of relative distances constantly widening, which is seen in all undulated belts. Besides, how could a simple upward pressure along a line in the crust, form a defined or limited anticlinal flexure? Whether the pressure were exerted by a liquid or a solid subterranean mass, it would produce rather a wide general moderate elevation, than a narrow, sharp, anticlinal wave.

If, again, this vague theory be modified to admit the action of a series of linear simultaneous pressures, coincident with the observed anticlinal flexures of an undulated district, it is not possible to understand why, being contiguous, they should not all conspire to lift the outer mass or crust into one general bulge or broad distended dome, rather than into a series of alternately synclinal and anticlinal waves. In addition to these difficulties, this notion of self-awakened lines of pressure, contains no clear hypothesis of the *origin* of the linear forces.

Hypothesis of Corrugation from Sinking of Tracts of the Earth's Surface.

Another theory of the cause of flexures in the Crust conceives them to have been produced from a sinking of the ground by removal of matter by volcanoes, or by the contraction of argillaceous rocks by heat and pressure. Sir C. Lyell, who appears to advocate this view, supposes that pliable beds may, in consequence of unequal degrees of subsidence, become folded to any amount, and have all the appearance of having been compressed by a lateral thrust; and the creeps in coal-mines are adduced as affording an excellent illustration of this fact.* With every respect for this eminent geologist's ingenious views, I must confess that this conception seems to me quite as much beset with difficulties as the somewhat kindred theory of elevation and simple upward protrusion. Apart from the objections that it supplies no cause for the peculiar shape of the crust waves, nor any explanation of their parallelism, and their remarkable laws of gradation, it appears to me quite inadequate to account for lateral corrugation at all, or for more than a very insignificant amount of it. A downward pressure or tension over a single area, produced by release of support arising from vacuities beneath the surface, ought not to engender, on any known mechanical principle, a series of flexures, either within or around the area, but should result in a mere subsidence or flattening of the portion from whence the support has been withdrawn. If the centering of a very flat dome, too weak to sustain itself, be removed, the dome either suddenly collapses with a fracture, or it indents itself, and sinks where it is weakest and most yielding, till it meets the supporting floor. Before the wide nearly level dome of a segment of the earth's crust can corrugate either itself or the adjoining strata, some alternate upward and downward force must undulate them, or they must contain alternate weak and strong belts, and even then these must be somewhat undulated; none of which conditions the hypothesis of subsidence is prepared to supply.

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* See Lyell's Elementary Geology, 5th Ed., p. 50.

Hypothesis of a simple Horizontal Compression.

A somewhat favourite and familiar mode of accounting for the undulation and plication of strata, is that which assumes them to have been corrugated by a purely horizontal or tangential pressure, without elevation and without pulsation; and this imagined mode of folding has been ingeniously illustrated by Sir JAMES HALL, Sir H. DE LA BECHE, and other geologists, by their placing flexible layers of clay, or cloth, or other substances, horizontally under a weight in a trough, and forcing one or both ends towards the centre, so as to contract the length of the strata, and thereby produce a series of miniature plications. It has been alleged that this folding of the clay or cloth is an exact imitation of the flexures of strata seen in nature; but I must deny the assumed analogy. The plications thus produced are merely irregular contortions; they exhibit no definite form of curvature, no constancy in the direction of their gentler and steeper slopes, and no law of regular gradation. Their anticlinal and synclinal axis planes, if they can be said to have any, lean some one way and some another; and the flexures, when the crowding is great, have a tendency to the horse-shoe form, and not to that of waves.

This hypothesis of corrugation, while erroneous in thus failing to present a true representation of the waves of the crust, is also defective in its mechanical principles, for it assigns no cause for the origination of the wave structure. A purely lateral or horizontal force should, as already intimated, simply bulge out to a feeble extent the whole compressed arch, but ought not of itself to wave it; some independent agency, producing alternate upward and downward flexure is indispensable to give even the most powerful tangential pressure the ability to plicate the flexible mass. This hypothesis is furthermore imperfect in not suggesting any cause in nature for the assumed horizontal pressure. It has been already shown, when discussing the hypothesis of simple elevation, and of simple subsidence of areas of the earth's crust, that neither of those movements, unaccompanied by an actual pulsation of the strata, would be competent to corrugate the crust at all; the pure elevation of an igneous axis having the tendency to stretch rather than compress the adjoining strata; and the simple sinking of an area, by retreat of support beneath, having only the effect to irregularly warp the surface, but in nowise to undulate it.

VIEWS OF GEOLOGISTS CONCERNING CLEAVAGE AND FOLIATION.

Professor **SEDGWICK**, as early as 1822, discovered and subsequently publicly taught the true nature of slaty cleavage, distinguishing it from joints, and showing it to be a tendency to separation in perfectly parallel planes, which are irrespective of the bedding. He ascertained that the slaty cleavage is usually confined to the finer-grained rocks, alternating coarser beds possessing it very imperfectly, and laid it down as a rule, that the *strike* of the cleavage is nearly coincident with the strike of the beds He referred it to crystalline or polar forces acting simultaneously and somewhat uniformly in giving directions. Subsequently, Professor **SEDGWICK** in 1835,* after many additional observations on the modifications of slaty cleavage, showed that the rule admitted of many limitations, which the geologist is compelled to notice in working out the structure of complicated districts. In a recent publication, his "Synopsis of the Classification of the British Palæozoic Rocks," he shows conclusively, that the cleavage structure is "the compound effect of all the crystalline forces acting on the mass, and that it cannot be due to a mechanical action." In this work, he also mentions the important fact of the existence frequently of a second cleavage plane, generally inclined at a great angle to the primary.

Sir J. **HERSCHEL** has suggested that the rocks possessing cleavage may have been so heated as to allow a commencement of crystallization, or heated to a point at which the particles may have begun to move among themselves, or on their own axes; surmising that some general law has determined the positions on which the particles have rested on cooling, and that this position has had some relation to the direction in which the heat escapes.†

Professor **PHILLIPS**‡ has shown, that in some slaty rocks, fossil shells and trilobites have been much distorted by cleavage; and he imputes this to a creeping movement of the particles of the rocks along the cleavage planes. This displacement, uniform over the same tract of country, he states to be as much as a quarter or even half an inch. Hard shells are not thus affected, but only the thin ones. Professor **PHILLIPS**, in 1843, stated that the cleavage planes of the slate rocks of North Wales are cleavages parallel to the main direction of the great anticlinal axes.

Mr **DANIEL SHARPE** conceives that the present distorted form of the shells in certain slates, has been produced by a compression in a direction perpendicular to the planes of cleavage, and an expansion in the direction of the cleavage dip.§

He conceives that the planes of cleavage range vertically along certain lines or belts, and dip towards those lines on each side of them; those nearest the central vertical belts at high angles, the angles gradually diminishing as

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[*] Geol. Trans., 2d Series, iii., p. 461.

[†] LYELL's Manual, p. 610.

[‡] Report British Association, Cork, 1843.

[§] Quarterly Jour. Geol., viii., p. 87.

the distance from the vertically dipping cleavage increases. This is his explanation of the fan-like arrangement of dip noticed in some countries. "This regularly descending series of planes being found on each side of parallel lines of vertical cleavage, the two series either meet in the centre in a sort of anticlinal axis, or coalesce into an arch. The planes between two lines of vertical cleavage appear to form a complete whole, and the area bounded by the vertical cleavage, may be considered as belonging to one system of cleavage, and may be called an area of elevation of the cleavage." He thinks the cleavage planes are really parts of great curves, which, if completed, would represent a series of semicylinders turned over a common axis.

Mr SHARPE thinks "that there is reason to believe that all slaty rocks have undergone a compression of their mass in a direction perpendicular to the planes of cleavage," connecting with this view his supposition that the cleavage areas are great anticlinal waves. He supposes that the compression of the slaty mass, and its expansion in the direction of the cleavage dip, have been due to the stretching of the strata in the direction of the curve representing the cleavage dips.

Mr CHARLES DARWIN,* reviewing his observations on cleavage in South America, says,—"The cleavage laminæ range over wide areas with remarkable uniformity, being parallel in strike to the main axes of elevation, and generally to the outlines of the coast." He recognizes the fact that the cleavage planes frequently dip at a high angle inwards, and he cites an instance of cleavage dip, in the mount at Monte Video, where "hornblendic slate has an east and west vertical cleavage, with the laminæ on the north and south sides near the summit dipping inwards, as if the upper part had expanded or bulged outwards." Mr DARWIN first proposed the term foliation for the laminæ in gneiss and other crystalline rocks, or the alternating layers or plates of different mineralogical composition. He pointed out the parallelism of the planes of foliation of the mica schists and gneiss with the planes of cleavage of the clay-slate in Tierra del Fuego and Chili, as seen by him in 1835. DARWIN conceives that foliation may be the extreme result of the process of which cleavage is the first effect, or that the crystalline form may have been most energetic in the direction of cleavage. He further suggests, "that the planes of cleavage and foliation are intimately connected with the planes of different tension to which the area was long subjected, after the main fissures or axes of upheavement had been formed, but before the final cessation of all molecular movement," "and that this difference in the tension might affect the crystalline and concretionary processes."

Mr SORBY, adopting the mechanical theory of cleavage, maintains that it varies directly as the mechanical changes, and inversely as the chemical (molecular) changes, which the strata have undergone. He thinks he has shown that the cleavage of certain limestones, microscopically examined by him, varies directly as the amount of mechanical compression to which they have been subjected, and that this compression was such as would necessarily change the structure of uncleaved into cleaved rock. He alleges "that cleaved limestones possess no crystalline polarity," and that in place of crystallization producing slaty cleavage, it has a contrary tendency, and, when perfect and complete, obliterates it altogether. Mr SORBY conceives that the absolute condensation of the slate rocks amounts, upon an average, to about one-half of their original volume.* This condensation he ascribes to the forcing together of the particles, and the filling up of their interstices by pressure perpendicular to the cleavage, and partly by elongation in the direction of the cleavage dip.

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* See Geological Observations on South America.

Mr DAVID FORBES,† writing upon foliation in rocks, leans to the conclusion that foliation is a distinct phenomenon from cleavage, and that the causes producing them were also distinct. He refers the foliation to chemical action, the cleavage to mechanical pressure. He admits that the planes of foliation and those of cleavage are often parallel to one another.‡ But the parallelism of the foliation to the cleavage he ascribes to a previously induced cleavage structure facilitating crystalline lamination in its own planes.

He supposes foliation to have resulted from a chemical action combined with a simultaneous arranging molecular force, developed at heats below the semifusion of the mass; also that the arrangement of foliation is often due to the proximity of igneous rocks, and tends to follow the direction of any lines in the rocks where the cleavage stratification, *or striæ of fusion,* follow preferably those lines offering least resistance.

Examination of the Prevailing Theories of Cleavage and Foliation.

From the theory of the origin of cleavage by mechanical compression exerted perpendicularly to the cleavage planes, as adopted by Mr SHARPE, Mr SORBY, Mr DAVID FORBES, and other geologists, I am constrained to dissent, and upon the following grounds:—

1. It has been already shown, in the general description of the phenomena of cleavage, that this tendency of fissuration is stronger and weaker in alternate closely contiguous planes, and is not diffused equally, even in the one direction, through the mass. Now it is impossible to conceive how a purely mechanical compression could have occasioned a regular alternation of greater and less condensation of particles, all equally free to move and adjust themselves into positions of statical equilibrium, and all equally subjected to the same amount of force. The well-known law of a *quaquaversal* tension of fluids is manifestly applicable to partially soft and flexible rocky matter, if we are to impute to this an

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* LYELL, p. 612.

† See his Paper, Quarterly Journal, Geological Society, 1855.

‡ See his Paper for a good figure of deflection of cleavage and foliation in the margin of a vein of quartz.

actual rotation of its parts, such as the mechanical theory assumes; and I cannot see why one uniform condition of aggregation should not be the result.

2. In the second place, it assigns no reason for the presence of cleavage planes in fine-grained argillaceous and calcareous rocks, and their absence in silicious ones, both fine-grained and coarse, even when the two classes alternate with each other in intimate parallel contact, where they must have been exposed to precisely the same pressure, both in direction and in amount. In other words, there is no relation discoverable between the known susceptibility of different materials to cleavage, and their susceptibility to compression. But on the other hand, some of the most compressible are the least subject to this peculiar structure. The different susceptibilities of different kinds of mineral matter to molecular polarity is, I conceive, the true explanation of this marked contrast in rocks.

3. Another quite conclusive objection, I conceive, to the pressure theory of cleavage is, that it fails to show how the cleavage-traversed strata can have received the pressure in one constant direction, and under an equalized intensity, through all the contortions and bendings which we know they must have possessed before cleavage was imparted to them. It is obvious that no mechanical pressure, come from what quarter it might, could transmit itself uniformly through convex and concave curves, through bodies of rock placed edgewise and flatwise towards it; but, on the contrary, dynamic considerations must convince us that the resultants of such a pressure would be as various in their directions within the mass, as the ever-changing planes of the corrugated stratification. Not only would the posture of the strata at any point next the quarter of the primary pressure influence the form and direction of the resultant planes of pressure at that point, but the differences in pliability of the different layers compressed would greatly modify them. In other words, while the dip of the cleavage planes within even wide limits, is usually remarkably constant, whatever the contortions of the strata, any pressure transmitted through these contortions must be as various, in different portions of the flexures, as the innumerable resultants produced by the ever-varying resistances and the pressure combined.

4. A like difficulty opposes itself to the pressure theory, in the constancy of the direction of the elongation or stretching of the mass in the line of its cleavage dip. This extension, well expressed by Professor PHILLIPS as a "creeping movement of the particles," seen not only in the fibrous grain of cleavage slates, but in the distortion of imbedded fossils, and of the whole substance of the rock indeed,—ascribed to mere compression by the authors above cited, but attributable, I think, to an actual molecular movement of the mass, in obedience to crystallizing polar forces,—is so equally graduated in amount, and so wonderfully constant in direction (never deviating much from the line of dip of the cleavage plane), that it could never have acquired this constancy from a merely lateral mechanical force, liable to infinite modification, in both of these

respects, by the continually varying resistances consequent on the contortions of the beds.

5. A further objection lies against the pressure theory, in the contradiction it offers between the direction which it assumes the compression to have come from, and the direction in which we can demonstrate the strata to have been actually pressed and moved. In every district of plicated and undulated strata, it can be shown, from the shape of the waves, from the declension in their curvature and height, from their mutual recession, from abatement in all the metamorphic signs of igneous action, and, finally, from the direction of the great planes of fracture in the crust, that the movement and pressure were upward and forward from the quarter of chief crust disturbance. Now it is nearly at right angles to this established direction of the forces, that the hypothesis I am reviewing assumes a pressure to have been applied to produce the cleavage. The planes of fissuration dipping inward towards the igneous side of the belt, any cleavage-producing pressure to be perpendicular to these planes, as the theory alleges it was, must have come either from a point or line elevated at least 45° above the earth's surface, or else from a point or region far below the earth's crust on the opposite side, or in the quarter where the cleavage is absent, or is invariably the least distinct, and where the flexures of the strata, and all other evidences of crust movement, are vanishing. This is, I conceive, a dynamic dilemma in which the compression theory finds itself,—either to make the force emanate from a quarter external to the crust entirely, or from just that quarter where we have the fullest evidence of the absence of any force at all. Thus, if the theory is applied to explain the south-dipping cleavage of the northern flank of the Alps, it implies either that the pressure came, not from within the crust below the crest of the chain, but from some point in the air high over the summits of the mountains, or else from some deep-seated subterranean region far to the north of the Alps, under the undisturbed plains of Northern Switzerland or Germany. In the case of the Appalachians, it requires that the pressure should have come, not from under the convulsed and ruptured region of the Atlantic slope, but from some high aerial point above this, or else from a spot diametric to it, deep under the plains of the Western States, where neither cleavage, metamorphism of any kind, nor undulations of the strata exist, to indicate the former presence there of any compressing force at all. (*See Sections of the Appalachians and Alps.* Figs. 1 and 3.)

6. Besides this general difficulty, I have a special one to offer connected with the laws of cleavage dip. This applies not only to the theoretical generalization of Mr DANIEL SHARPE respecting the relations of the cleavage planes to each other in different parts of a zone of slaty cleavage, but to the observations upon which his generalization has been built. His sections of the cleavage in North Wales and elsewhere, represent it as perpendicular or steepest in the belts of maximum igneous action, and flattest in the regions most remote from these, where he places

the anticlinal axes of his cleavage curves. Now, just the reverse of this steepening of the cleavage planes towards the regions of chief metamorphism, will be found to be the real law of gradation in the Appalachians, the Alps, and the district of the Ardennes and Southern Belgium. Obedient to a law already explained, the cleavage dip, following the dip of the axis planes of the flexures, is not most but *least* inclined in the districts most convulsed, and grows progressively steeper, as we advance across the undulations to the districts of minimum disturbance. In the Alps the plications lie flattest next the high central crests of the chain, and there the cleavage dip is often at a very low angle; but receding towards the plain of Switzerland, where the theoretical view requires that it should be flatter, it is really steeper, and even approaches to perpendicularity; and precisely analogous is the gradation when we cross the Appalachians from south-east to north-west. Generalizing the dips of the cleavage planes on both sides of a double belt of flexures like that of the Alps, and excluding the central crests, where the jointage of the igneous rocks, and the cleavage structure impressed by them is more vertical, the real curve of dip for the whole zone will be found to be a *synclinal* one, and not the two halves of two anticlinals, the generating axes of which are far outside the chain, one in the plain of Switzerland, the other in the plain of Northern Italy.

I am much gratified to find, that my objections to the mechanical theory of cleavage find support in the able writings of Professor SEDGWICK, who, in a note in his "Synopsis," states several cogent reasons for rejecting the hypothesis. While some of my own objections are but an expansion of those presented by this eminent geologist; others are independent of his, growing out of my own observations. This accordance gives me additional confidence in the soundness of the generalizations upon which they rest.

THEORETICAL VIEWS.

Theory of the Flexure and Elevation of Undulated Strata.

The wave-like structure of the Appalachians and other undulated zones, has been attributed by the author and his brother, W. B. ROGERS, in their communications to the American Association in 1842, and to the British Association in the same year, to an actual undulation of the supposed flexible crust of the earth, exerted in parallel lines, and propagated in the manner of a horizontal pulsation from the liquid interior of the globe. We suppose the strata of such a region to have been subjected to excessive upward tension, arising from the expansion of molten matter and gaseous vapours, the tension relieved by linear fissures, through which much elastic vapour escaped, the sudden release of pressure adjacent to the lines of fracture, producing violent pulsations on the surface of the liquid below. This oscillating movement in the fluid mass below would communicate a series of temporary flexures, to the overlying crust, and these flexures would be rendered permanent (or keyed into the forms they present) by the intrusion of molten matter. If, during this oscillation, we conceive the whole heaving tract to have been shoved (or floated) bodily forward in the direction of the advancing waves, the union of this tangential, with the vertical wave-like movement, will explain the peculiar steepening of the front side of each flexure, while a repetition of similar operations would occasion the folding under, or inversion, visible in the more compressed districts. We think that no purely upward or vertical force, exerted either simultaneously or successively along parallel lines, could produce a series of symmetrical flexures, and that a tangential pressure unaccompanied by a vertical force, would result only in an imperceptible bulging of the whole region, or an irregular plication dependent on local inequalities, in the amount of the resistance. The alternate upward and downward movement necessary to enable a tangential force to bend the strata into a series of regular parallel subsiding flexures has been, we conceive, of the nature of a pulsation, such as would arise from a succession of actual waves rolling in a given direction, beneath the earth's crust. It is difficult to account for the phenomena, by any hypothesis of a gradual prolonged pressure exerted either vertically or horizontally. The formation of the grand, yet simple flexures so frequently met with, cannot be explained by a *repetition* of feeble tangential movements, since these could not successively accord, either in their direction or in their amount, nor can it again, by a repetition of merely vertical pressures, for it is impossible to suppose that these could, without some undulating action, shift their positions through a series of symmetrically disposed parallel lines. We find it equally impossible to understand how, if feeble and often repeated, these vertical pressures should always return to the same lines to produce the conspicuous flexures we behold. The oscillations of the crust to which the undulations of the strata are attributed have been, we conceive, of the nature of the Earthquakes of the present day. Earthquakes consist, as we think we have demonstrated, of a true pulsation of the flexible crust of the globe, propelled in parallel low waves of great length and amplitude with prodigious velocity, from lines of fracture, either conspicuous volcanic axes, or half concealed deep-seated fissures, in the outer envelope of the planet.

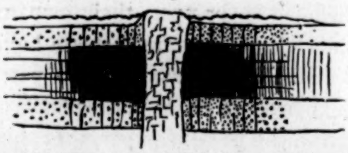
Theory of Cleavage Structure.

Concerning the cause of slaty cleavage, I have adopted the explanation originally proposed by Professor SEDGWICK, that it is due to crystalline or polar forces acting simultaneously and somewhat uniformly in given directions on large masses having a homogeneous composition. And following up the further suggestion in extension of this idea ingeniously proposed by Sir JOHN HERSCHEL, that this molecular force was of the nature of an incipient crystallization, and has been developed in the particles, by their being heated to a point at which they could begin to move among themselves, or upon their own axis, I have endeavoured to show, that whether the cleavage-traversed strata have been much disturbed or not, the cleavage planes invariably approximate to parallelism with those great planes in the crust, which appear to have been the planes of maximum temperature. It has been already stated in the present paper, that the cleavage dip is parallel to the average dip of the anticlinal and synclinal axis planes, or those bisecting the flexures. Now, it is easy to prove, that these axis planes, and the inverted parts of the flexures, are just those portions where the greatest wrenching, fissuring, and opening of the strata must have occurred, and where the highly-heated, pent up, volcanic steam and gases, and liquid mineral matter, must have found their chief channels upwards to the surface.

Without attempting at present to apply this doctrine in detail, I will content myself with reviving a suggestion I formerly put forth, that every plicated belt of strata may be looked upon as having, from the causes here adverted to, become traversed at the time of their folding and metamorphism, by a series of alternate hotter and cooler parallel planes or zones of temperature, arranged in oblique dip, coincident approximately with the axis planes of the flexures. These planes or surfaces of high temperature, we may suppose to have acted to polarize the particles in corresponding planes, by transmitting through the half-softened mass, a succession of parallel waves of heat, stimulating the molecular crystallizing forces, which are ever resident in mineral matter, and which only await there the quickening influence of such a temperature, to develop in the mass special lines and surfaces of maximum and minimum cohesion.

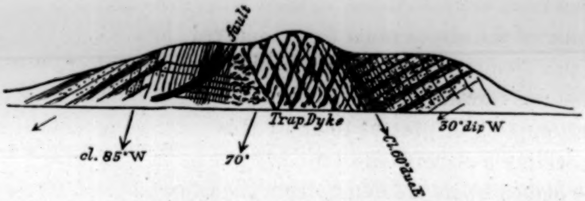
This conception, that the surfaces or planes of crystalline lamination, including cleavage, which is but a lower grade of the same species of molecular metamorphism, are approximately parallel to the surfaces of the waves of temperature, which have moved through the strata, is not a mere hypothetical speculation, but an induction at which I have arrived, from a comparison of many observations of my own, with phenomena well recorded by the ablest geologists. Nearly all observers who have noted the influence of igneous dykes and veins upon the strata adjoining them, both in mines and external exposures, have seen a more or less distinct lamination or cleavage adjoining the walls of the once heated mineral matter, and have been struck by its very general parallelism, to the surface or the axis of the vein. Cases occur in strata of all ages, and are frequently brought to light in coal-fields, when nearly vertical dykes cutting low dipping or horizontal shales, susceptible of the cleavage metamorphism, have occasioned in the latter a true cleavage perpendicular to the stratification, or parallel, more strictly speaking, with the once hot surface of the intrusive rock.

Fig. 7.



Cleavage in Red Shale caused by a Trap Dyke parallel to its walls, near New Hope, Pennsylvania.*

Fig. 8.



Cleavage superinduced by a Trap Dyke in Red Argillaceous Sandstone of Jurassic age, west of Gettysburg, Pennsylvania.

Other instances are often presented of masses of superincumbent trap rock, baking and altering argillaceous and other strata, in which a like law of parallelism of the cleavage to the heat-imparting surface of the molten matter is shown in the horizontality of the cleavage planes, whatever be the dip of the strata. Numerous examples can be cited, where one igneous dyke cutting another, or traversing a mass of earlier Plutonic rock, produces in the latter a crystalline grain, amounting to a sort of cleavage, adjoining the bounding surfaces of the newer injection, and in planes invariably parallel, or nearly so, to the walls of the fissure. A similar fact of the occurrence of a cleavage parallel to the walls of highly heated fissures, may be seen in the faults and great dislocations which traverse some parts of the anthracite coal basins of Pennsylvania. Here the greatly indurated argillaceous shales, and even sometimes the coal itself, display a cleavage-structure invariably parallel to the general plane of the fracture. Such fissures would be the natural channels through which heated volcanic steam would ascend from the interior, and the action of this upon the strata most susceptible of cleavage would be precisely analogous to that of a molten dyke, in transmitting a wave of heat perpendicular to its surface, partially softening and half polarizing the matter as it passed.

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* There is a similar instance cited by Professor PHILLIPS, I think in his Geology of Yorkshire.

General Resumé.

1. *Wave-like form of all belts of uplifted strata.*

a. It is a general fact that strata dip in curved and not in straight planes.

b. Wherever wide areas of the crust have been elevated or depressed from the level at which their strata were deposited, these strata will be found, except where their dip is disordered by crust dislocations, to constitute, in their varying angles of dip, one or more wide regular curves.

2. *Parallelism of crust undulations.*

a. It is another general fact, that these undulations of the strata are in the form of long parallel waves, resembling much those great continuous billows called in dynamics waves of translation, and by seamen rollers.

3. *Relations of flexures.*

a. Parallelism of the waves to the general trend of the part of the mountain system to which they belong, and especially to its chief igneous axis.

b. Parallelism of flexures extends not only to adjacent individual waves, but to contiguous groups, and is as true of curvilinear as of straight.

c. The waves of the strata are generally of two or three grades of magnitude, as respects their length, height, and amplitude, and while those of the same grade are parallel, the different grades are not necessarily so.

4. *Laws of form and gradation of waves.*

There are three characteristic forms of crust waves; symmetrical flexures equally steep on the two slopes; normal flexures, curving more rapidly on one side than on the other; and folded flexures, or those with a doubling under of their more incurved slopes, and among which the steepest slopes are generally directed to the same quarter.

The geometric planes bisecting the anticlinal and synclinal bends of the strata, here called axis planes, are nearly perpendicular in the symmetrical waves, but inclined in the other two classes, dipping at the lowest angle in the folded flexures. In many belts the plication is such as to amount to parallelism of all the inverted to the uninverted sides of the waves.

Some waves are straight, some curvilinear and crescent shaped, and many of them extremely regular, changing their trend 40° or even 50°. The curvilinear ones convex *from* the disturbed sides of the zones, are generally more regular than those which are convex *towards* them.

5. *Gradations in flexures.*

   a. In all undulated zones, the succession, starting from the most disturbed side, is invariably from the folded waves to the unequally sloping or normal ones, and from these to the equally sloping or symmetrical.

   b. The waves grow progressively wider apart, or increase their amplitude, as they pass from the folded to the equally sloping form.

   c. The waves progressively flatten down as they recede from the folded side of the belt.

   d. The axis planes of the flexures of any great undulated zone all incline towards the same quarter, that of maximum disturbance, the angle of inclination being less the nearer the wave or plication is to that side.

6. *Fractures or faults.*

In undulated districts, the dislocations are of two kinds: (1.) Numerous short ones, transverse to the strike of the axes, and shifting the strata to but a trivial extent; (2.) longitudinal ones, fewer in number, of great length, and producing often great displacement.

The longitudinal faults very generally dip towards the same quarter as the axis planes; indeed they are either ruptures in the axis planes of the flexures, or in the steep or inverted sides of the waves.

This slanting of the plane of dislocation parallel with the leaning of the wave, causes the newer or upper formations, on the inverted side, to dip under the older or lower on the uninverted side of the flexure or the fault, for almost always the uninverted side has been shoved forward and upward across the inverted.

Some undulated belts are single, or have all the axis planes of the flexures dipping to one quarter, as the Appalachians and the zone of Southern Belgium. Others are double, or consist of two such zones, both dipping inwards towards one central line of chief igneous disturbance, and these latter present in this inward general leaning a fan-like structure, as in the Alps.

7. *Phenomena of slaty cleavage.*

   a. The cleavage dip is independent of the dip of the strata; and still more remarkably, the cleavage planes of a district are generally parallel to the axis planes of its flexures.

   b. Immediately within the anticlinal and synclinal axes, the cleavage planes depart from their parallelism to the axis planes to dip inwards towards them in a kind of fan-like arrangement.

   c. The cleavage is only present where the rocks consist of certain materials, abounding most where they are most argillaceous and of finest texture, disappearing and reappearing with changes in the composition of the strata, even where this closely alternates.

d. In such groups of alternating cleavable and non-cleavable beds, the cleavage planes curve from the normal dip they possess to approach to a parallelism with the planes of separation of the strata as they near their surfaces.

e. The cleavage susceptibility is alternately greater and less in closely adjacent parallel planes.

The ribbon structure of glaciers is probably analogous to the cleavage structure of argillaceous rocks.

8. *Foliation.*

In districts of crystalline, metamorphic, or gneissic strata, not much disturbed or corrugated, the foliation generally coincides with the stratification. In regions much corrugated the foliation, on the contrary, is often at a steep angle to the stratification, and shows a tendency to dip, as cleavage does, parallel to the axis planes of the flexures. Generally the direction of the foliation appears to conform to that which the waves of heat metamorphosing the rocks would take in slowly flowing through them.

9. *Theories of elevation.*

a. A common hypothesis of the cause of the elevation of strata is that of a wedge-like intrusion of melted matter. But this implies a function in semifluid or fluid matter incompatible with the dynamic properties of liquids. Some force must have first cracked the strata before the molten rock could insert itself. Veins and dykes tapering upward do not belong to lines of anticlinal elevation, where geologists so frequently indicate them, but to synclinals or concave curves.

b. The kindred idea of the intrusion of igneous rocks in solid wedges separating and lifting the crust is also at variance with sound mechanical laws. To exert this lifting and thrusting force, the assumed wedges must have moved freely through the fissures they fill; but we see no proofs of discontinuity between the igneous and stratified rocks, but evidences of the closest cohesion.

c. A modified view of the wedging up of the flexible strata, conceives them to have been simply carried up by the lifting of the igneous nucleus. Such movements have no doubt occurred, and have served to steepen the strata leaning against the igneous rocks, but they cannot have corrugated the strata, which would be rather stretched than compressed by the elevation.

d. The hypothesis of a simple upward pressure at points or lines in the crust, which does not include an explanation of the wave structure of disturbed districts, cannot be a true theory; it must show how the pressures have shifted to new and parallel lines, and lines constantly receding, and also show why, if the linear pressures were simultaneous, they should not have produced a wide general arching, rather than a series of contiguous sharp waves.

e. The hypothesis of the origin of flexures from a sinking of the ground by removal of volcanic matter beneath it, supplies no explanation of the origin of zones of regular undulations. The sinking of any weak segment of the earth's crust might produce a trivial general warping, but not a belt of waves.

f. The hypothesis of a simple lateral or horizontal compression, illustrated by the folding of layers of any flexible material squeezed edgewise, and kept down by a weight, is open to the objection that this mode of folding offers no true analogy to the great symmetrical parallel flexures met with in nature. Flexures thus artificially produced, show neither the forms nor the gradations characteristic of the crust waves. A purely tangential force would cause the district within its influence to bulge slightly upward, but not to corrugate into regular undulations, and it fails to find an origin for a pressure in the direction assumed.

10. *Theories of cleavage and foliation.*

a. The prevailing notions of geologists respecting the origin of cleavage and foliation are, on the one hand, that they have been produced by different intensities of molecular crystallizing polarities, excited by heat operating in a definite direction, on the other hand, that they have been caused by mechanical compression of the strata applied perpendicularly to the cleavage and foliation planes.

b. One main objection to the pressure hypothesis is, that it does not account for the existence of planes of alternately stronger and weaker cohesion.

c. Another difficulty is, that it fails to explain the dependence of cleavage upon the texture of the rock, especially its chemical nature, and particularly where cleavable and non-cleavable strata alternate in close contact.

d. A third important objection exists in the dynamic difficulty, that the cleavage is nearly parallel and constant in its dip, despite the inequalities which a lateral pressure should undergo in its transmission through all the contortions and various postures prevailing in the strata.

e. A fourth difficulty, analogous to the last, is presented by the constancy in the amount and direction of the elongation or creep of the cleavage rocks in the direction of their cleavage dip; a constancy not compatible with the ever-varying tension which the flexures and bendings of the strata would occasion.

f. An additional objection presents itself, in the direction of the pressure implied by the theory, which, assuming the force to have been perpendicular to the cleavage planes,i mplies it to have come either from a source above the earth's surface, on the side towards which the cleavage planes are dipping, or from a source far beneath the crust, in that quarter where invariably the cleavage and all other symptoms of metamorphism are least abundant, or entirely wanting.

g. Still another important objection arises, in the contradiction exhibited between the law of gradation in the steepening of the cleavage dip demanded by the theory, or one form of it, and the actual law of the gradation of this dip witnessed in all districts of regular crust plications. The theory represents the cleavage dip as growing progressively steeper the nearer it is to the lines of greatest igneous action, the facts in nature show that the cleavage, the foliation, and the axis planes of the flexures, with which these are approximately parallel, grow progressively steeper the farther they recede from those lines of maximum energy.

Concluding theoretical views.

The wave-like structure of undulated belts of the earth's crust is attributed to an actual pulsation in the fluid matter beneath the crust, propagated in the manner of great waves of translation from enormous ruptures occasioned by the tension of elastic matter. The forms of the waves, the close plication of the strata, and the permanent bracing of the flexures, are ascribed to the combination of an undulating and a tangential movement, accompanied by an injection of igneous veins and dykes into the rents occasioned by the bendings.

This oscillation of the crust, producing an actual floating forward of the rocky part, has been, it is conceived, of the nature of that pulsation which all great earthquakes produce at the present day.

11. *Cleavage.*

The cleavage planes having been shown to be parallel to the axis planes of the flexures, and locally to the planes of the great faults, and these being obviously the belts of maximum temperature in a plicated district, it is suggested that both cleavage and foliation are due to the parallel transmission of planes or waves of heat, awakening the molecular forces, and determining their direction.

Postscript.

From my friend, Professor GOODSIR, who recently (June 27th) delivered a lecture of great interest and originality, on the Retina, to the Anatomical Students of the University of Edinburgh, I learn that, as he stated to his hearers, his microscopical observations on the structure and development of the eye had led him to the conclusion, that *only* the rays of light which are returned from behind through the retina produce a luminous sensation, and that the objective perception of light commences physically towards the choroidal, not the hyaloid extremity of the optically sensific constituents of the retina.

According to BRÜCKE (as mentioned in the text, Note, p. 334), the bacillar layer acts as a mirror, reflecting light forwards, and luminous sensation begins in a layer of grey nervous substance, situated nearer the front of the retina,—an opinion combated by KÖLLIKER.

According to Mr GOODSIR, the objective perception of light begins somewhere near the junction of the rod or cone with the Müllerian fibre (see Note, p. 332). On this view, the entire arrangement of rod or cone, with its Müllerian filament, is not a nerve-structure, as KÖLLIKER holds, but a peculiar organ referable to the same category as the tactile corpuscles and Pacinian bodies, and so constructed as to oppose the extremity of the nerve, which is contained in it, to the ray of light passing backwards from the choroid along the axis of the rod or cone, so that the ray shall impinge upon its extremity in the line of its axis, this being, according to Mr GOODSIR's hypothesis, the only direction in which a luminous ray can optically affect a nervous filament.

I have argued, in the preceding paper, for such returned light being *accessory* to vision, but according to this view it is the only light by which it is exercised. If this doctrine (however modified in details) be established, the reflection of light from the choroid will prove to be *essential* to the functions of seeing, and the necessity for the living eye being a Camera Lucida will be based upon deeper grounds of proof than I have attempted to offer.

August 10, 1855.

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